

MONTHLY NOTICES

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No. 6

Lient.-General J. F. TENNANT, R.E., C.I.E., F.R.S., President,
in the Chair.

John Billington Booth, J.P., Preston, Lancashire; and
Hannibal Mirabita, 134 Strada Britannica, Alexandria,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as
Fellows of the Society, the names of the proposers from personal
knowledge being appended :—

Richard Edward Syngé Cooper, Gentleman, Hillmorton
Paddox, Rugby (proposed by J. C. Adams);

Hugh Frank Newall, Trinity College, Cambridge (proposed
by H. H. Turner); and

Samuel H. Ridge, B.A., F.R.G.S., 275 Victoria Parade East,
Melbourne, Victoria (proposed by P. Edward Dove).

Sixty-five presents were announced as having been received
since the last meeting, including amongst others :—

Annales de l'Observatoire de Paris (Mémoires), tome xix.,
presented by the Observatory; *Œuvres complètes de Laplace*,
tome viii., presented by the Academy of Sciences, Paris.

Sirius. By S. W. Burnham, M.A.

It may not be generally known, even by those who have given special attention to observing or investigating the *Sirius* system, that all the collections of the measures of the companion hitherto published are not only very incomplete with respect to the number of measures which have been made, but contain many material errors in the observations quoted and made use of. Errors of one sort and another, first made long ago, have been handed down, and, remaining unchallenged, have come to be overlooked altogether. Many years ago I undertook to collect all the measures that have been made of every known double star, and the more important notes and papers referring to them, and have since added the current measures and observations, so that the history of each double star is substantially complete to this time. I have endeavoured to make these references to *Sirius* cover the various points of interest connected with it as a double star, commencing with the investigation which resulted in the discovery of the companion by Mr. Alvan G. Clark, and especially to include all measures of the small star since the first observation in 1862. The time will soon come when a complete list of these measures will be more important than it has been heretofore, for the close determination of the real period of this interesting binary. Down to this time the result must necessarily be only approximate, since the companion has moved through less than 90° of its orbit, and the real dimensions of the apparent ellipse cannot be rigidly determined. In a few years more, and probably as soon as the companion can be seen by any telescope now in use, it will have added 180° to the arc already passed over, and then a single measure will enable the period to be computed with a very small percentage of error. This important additional measure I trust can be made with the Lick telescope within the next four or five years.

In view of the new interest which this system will then have for both the observer and the computer, I have thought it important to make as perfect a collection as possible of the published observations for the benefit of all concerned. In nearly every instance I have referred to the original publication, and there obtained the figures here given. In many cases this has been necessary to avoid the errors frequently made by taking the measures from second-hand sources. As an illustration of this, I may mention one or two examples.

In some of the collected observations, Challis is credited with having measured the companion in 1862. As a matter of fact he did not measure it, and, so far as I know, never attempted to. This measure was really made by Chacornac. Gledhill gives a different result to both Challis and Chacornac, which doubtless

led to a repetition of the same error by Gore and Howard in their computations of the orbit.

Each one of these authorities has given a set of measures by Mitchel as made in 1863. This is an obvious mistake, as it is well known that General Mitchel for some time previous to this was in the military service of the United States, and engaged in the more important work of suppressing the rebellion. He died in October 1862. These measures were really made by Marth, not in 1863, but in 1864. Marth did not observe *Sirius* in 1863 at all, although he is put down as having done so by the authors previously referred to.

Gore gives a measure in 1885 credited to the Paris Observatory; and Howard includes this with the measures used in computing a new orbit published a few weeks ago (Gould's *Astronomical Journal*, 235), and also gives a measure in 1884 from the same observatory. These two measures are as follow:—

1884·27	36°3	8''70	Paris Obs.
1885·11	34·1	8·09	Paris Obs.

Now these two measures are identical in date, angle, and distance with the observations of Young in 1884 and 1885, and, as I have found no authority elsewhere for these measures, I conclude that they should have been credited to Princeton instead of Paris.

To enable those who wish to consult these measures to do so conveniently, I have given, following the measures, a complete list of the works in which every measure cited can be found. The authors of the papers are given in alphabetical order, but the names of all the observers do not appear in this way, as in some instances the measures of other observers are included under one name. It should be remarked also that some of the references do not relate to micrometrical measures, but have some other special interest, and should be included in a list of valuable papers on *Sirius*.

The following are the measures I have collected:—

Measures of Sirius.

1862.

1862·19	84°6	10''07	Bond	3 ⁿ
1862·20	85·0	10·09	Rutherford	5 ⁿ
1862·23	84·5	10·42	Chacornac	2 ⁿ
1862·28	83·8	(4·92)	Lassell	1 ⁿ

1863.

1863·21	82°5	10''15	Otto Struve	2 ⁿ
1863·15	88·4	7·63	Secchi	1 ⁿ
1863·21	81·3	9·54	Rutherford	6 ⁿ
1863·23	84·9	10·00	Dawes	1 ⁿ
1863·27	82·8	...	Bond	1 ⁿ

1864.

1864.14	79°4	10°60	Marth	3 ⁿ
1864.18	80.1	9.63	Lassell	1-3 ⁿ
1864.22	78.6	10.70	Bond	4-2 ⁿ
1864.22	74.8	10.92	Otto Struve	6-3 ⁿ
1864.23	84.9	...	Dawes	1 ⁿ
1864.24	79.7	10.08	Winnecke	1 ⁿ

1865.

1865.21	77°6	10°59	Otto Struve	2 ⁿ
1865.22	75.5	9.59	Secchi	8 ⁿ
1865.23	77.8	10.77	Förster	5-4 ⁿ
1865.25	76.9	...	Tietjen	3 ⁿ
1865.26	76.0	...	Bond	...
1865.26	76.9	(9.0)	Engelmann	1 ⁿ .

1866.

1866.07	77°2	10°43	Knott	2-1 ⁿ
1866.21	...	10.74	Bruhns	1 ⁿ
1866.21	75.2	10.93	Otto Struve	3 ⁿ
1866.22	73.9	10.97	Tietjen	2-1 ⁿ
1866.23	74.1	11.29	Förster	3-1 ⁿ
1866.23	74.0	10.21	Hall	2-3 ⁿ
1866.23	74.9	10.57	Newcomb	3 ⁿ
1866.25	78.3	10.34	Tuttle	1 ⁿ
1866.26	74.7	10.09	Eastman	3 ⁿ
1866.29	71.3	10.11	Secchi	3 ⁿ .

1867.

1867.02	74°2	11°15	Winlock	7-6 ⁿ
1867.10	73.8	10.66	Searle	6-5 ⁿ
1867.22	72.1	10.98	Otto Struve	1 ⁿ
1867.24	72.3	...	Förster	2 ⁿ
1867.27	74.9	9.92	Eastman	2-1 ⁿ

1868.

1868.02	73°2	10°25	Searle	2 ⁿ
1868.04	72.1	...	Peirce	1 ⁿ
1868.23	70.3	11.25	Vogel	7 ⁿ
1868.24	69.6	11.35	Bruhns	5 ⁿ
1868.26	71.7	10.95	Engelmann	5 ⁿ

1869.

1869.10	74°7	10°26	Brünnow	7-4 ⁿ
1869.15	73°6	11°23	Vogel	3 ⁿ
1869.20	68°7	11°17	Dunér	1 ⁿ
1869.20	68°6	11°07	Winlock	2 ⁿ
1869.23	69°4	10°93	Peirce	1 ⁿ

1870.

1870.13	68°1	11°16	Peirce	12-4 ⁿ
1870.17	65°9	11°06	Winlock	7-5 ⁿ
1870.24	65°1	12°06	Vogel	5 ⁿ

1871.

1871.16	65°9	10°75	Secchi	3 ⁿ
1871.20	70°3	11°19	Peirce	2-1 ⁿ
1871.23	64°1	11°11	Dunér	2 ⁿ
1871.25	60°1	12°10	Pechüle	4-3 ⁿ

1872.

1872.18	59°8	11°05	Dunér	2 ⁿ
1872.24	62°4	11°50	Newcomb	1 ⁿ
1872.24	64°3	11°46	Hall	6 ⁿ
1872.26	61°3	...	Skinner	3 ⁿ

1873.

1873.20	65°8	11°12	Hall	1 ⁿ
1873.22	60°8	10°57	Dunér	1-4 ⁿ

1874.

1873.93	65°0	11°29	Wilson & Seabroke	1 ⁿ
1874.16	59°0	11°46	Newcomb	7 ⁿ
1874.19	58°7	10°99	Holden	2-1 ⁿ
1874.23	58°0	11°10	Hall	2 ⁿ

1875.

1874.83	57°5	...	Burton	1 ⁿ
1875.19	57°1	10°73	Dunér	4 ⁿ
1875.21	56°6	11°41	Newcomb	2 ⁿ
1875.21	55°9	11°89	Holden	5-4 ⁿ
1875.28	56°4	11°08	Hall	4 ⁿ

1876.

1876.03	57°8	11°12	Watson	1 ⁿ
1876.05	54.6	11.45	Peters	1 ⁿ
1876.09	54.9	11.82	Holden	6 ⁿ
1876.14	55.0	11.55	Russell	4 ⁿ
1876.22	55.2	11.19	Hall	6 ⁿ

1877.

1877.11	52°8	11°19	Cincinnati	4-3 ⁿ
1877.16	52.8	11.35	Holden	4 ⁿ
1877.26	53.4	10.95	Hall	5 ⁿ

1878.

1877.97	52°4	10°83	Burnham	8 ⁿ
1878.07	50.5	11.07	Holden	4 ⁿ
1878.15	51.0	10.71	Cincinnati	9 ⁿ
1878.19	54.4	11.24	Pritchett	5 ⁿ
1878.22	53.2	11.4	Eastman	...
1878.24	51.7	10.76	Hall	5 ⁿ

1879.

1878.70	50°0	10°61	Cincinnati	20-14 ⁿ
1879.05	50.7	10.44	Burnham	10 ⁿ
1879.12	47.8	11.35	Holden	5 ⁿ
1879.15	50.3	10.78	Pritchett	5 ⁿ
1879.20	50.1	10.55	Hall	6 ⁿ

1880.

1879.75	46°5	10°29	Cincinnati	1 ⁿ
1880.00	48.8	10.55	Russell	1 ⁿ
1880.10	47.1	10.48	Holden	4 ⁿ
1880.11	48.3	10.00	Burnham	11 ⁿ
1880.17	49.6	9.87	Hough	3 ⁿ
1880.22	51.1	...	Smith	1 ⁿ
1880.25	47.8	10.30	Hall	8 ⁿ
1880.28	48.6	10.38	Frisby	2 ⁿ

1881.

1881.07	46°3	9°77	Burnham	8 ⁿ
1881.12	43.3	10.83	Holden	2 ⁿ
1881.17	46.9	10.11	Frisby	6 ⁿ
1881.18	46.5	9.81	Young	7 ⁿ
1881.26	45.3	9.60	Hough	5 ⁿ
1881.26	45.3	10.00	Hall	6 ⁿ

1882.

1881.99	43°6	9''38	Burnham	11 ⁿ
1882.13	43.1	9.30	Hough	9 ⁿ
1882.18	42.2	9.95	Frisby	6 ⁿ
1882.23	42.5	9.67	Hall	7 ⁿ
1882.54	44.0	...	Engelmann	6 ⁿ

1883.

1883.10	40°1	9''05	Burnham	10 ⁿ
1883.10	39.0	9.41	Young	1 ⁿ
1883.12	39.7	9.02	Hough	11 ⁿ
1883.14	41.3	...	Wilson	4 ⁿ
1883.17	41.4	9.75	Frisby	7 ⁿ
1883.21	39.1	9.26	Hall	6 ⁿ

1884.

1884.05	36°0	9''67	Perrotin	6 ⁿ
1884.18	36.7	8.51	Hough	11 ⁿ
1884.19	36.4	8.39	Burnham	10 ⁿ
1884.23	37.7	8.81	Hall	8 ⁿ
1884.27	36.3	8.70	Young	5 ⁿ

1885.

1885.11	34°1	8''09	Young	8 ⁿ
1885.20	32.7	7.96	Hough	10 ⁿ
1885.27	34.7	8.06	Hall	8 ⁿ

1886.

1886.05	29°8	7''59	Young	4 ⁿ
1886.14	28.7	7.21	Hough	12 ⁿ
1886.22	30.6	7.39	Hall	6 ⁿ

1887.

1887.14	25°4	7''08	Young	4 ⁿ
1887.19	23.7	6.78	Hough	7 ⁿ
1887.23	24.2	6.51	Hall	4 ⁿ

1888.

1888.24	23°3	5''78	Hall	3 ⁿ
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1889.

1888·97	13°·9	5''·27	Burnham	5 ⁿ
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1890.

1890·27	359°·7	4''·19	Burnham	3 ⁿ
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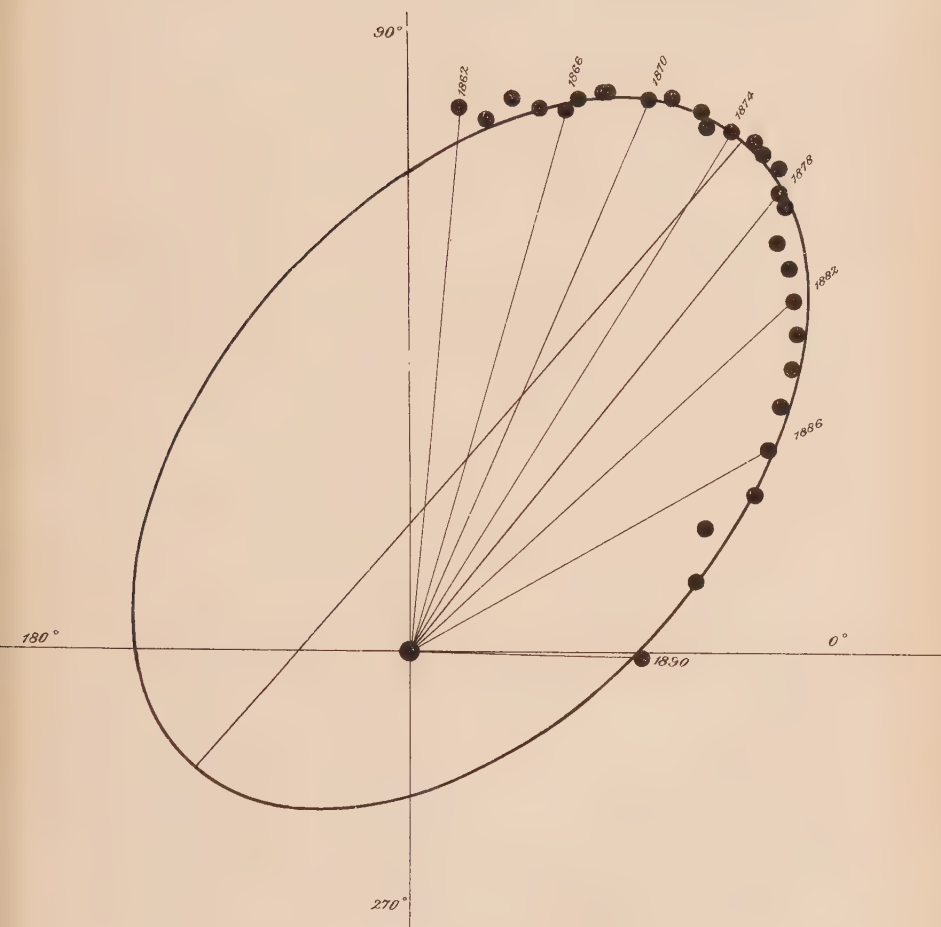
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The Period of Sirius.

Some early attempts were made to see how far the motion of the observed star agreed with the theoretical companion, but the arc described was so short, and the motion of the smaller star so nearly uniform, that no reliable result could be reached from the observations. An ellipse of almost any magnitude would satisfy the observed positions as well as one which agreed with that laid down for the theoretical star. In 1881 Plummer pointed out that a period of 440 years would represent the observations about as well as one of fifty years. Since that time the limits of the possible ellipses which could be drawn through the observed positions have been greatly narrowed by the later measures, and the uncertainty of the period of rotation correspondingly reduced.

Two orbits have been recently computed: one by Gore, using measures made each year down to 1889, who finds a period of 58.47 years; and the other by Howard, who includes the very last measures of the companion made here in 1890, and obtains a period of 57.02 years. Very few of the measures are used by the last-named computer, and some of the years are not represented at all. The measures are not always correctly given, and in one instance at least the error is a large one. This occurs in a set of measures made by me in 1877. The mean result was derived from measures on eight nights. Some of these were at the end of 1877, and some in the beginning of 1878. In the paper referred to these are given as two series: one embracing the observations of 1877, and the other those of 1878, with an error of 1" in the latter distance. Of course, these measures all belong together, and should be represented by a single mean, as given in the original publication.

The close agreement in the periods obtained by these independent investigations is worthy of note, and perhaps no better result could be got from the material available at this time. However, I have made a rough investigation in the same direction, which is given here for what it is worth. I have followed a plan which differs somewhat from that usually adopted in this: I have taken the complete series of measures given above, and deduced from the observations of each year a result which in my judgment would best represent, so far as the measures are concerned, the position of the companion. Of course this involved a consideration of the probable merits and value of each set of measures, as derived from the skill and experience of the observer, the size of the instrument used, the number of nights,



SIRIUS

and the general movement of the small star from the observations as a whole. It is not claimed that this method should be generally followed, since it would require a thorough familiarity with the relative value of measures by the different double star observers which many would have no opportunity of acquiring, but for the purpose I had in view in this instance it was certainly as good as any other.

The positions obtained in this manner are as follow :—

1862.20	85°0	10°08	1877.18	53°0	11°16
1863.22	82.9	9.90	1878.12	51.7	10.77
1864.20	79.6	10.38	1879.02	50.3	10.59
1865.22	77.0	10.32	1880.18	48.6	10.06
1866.21	74.5	10.39	1881.19	45.8	9.79
1867.17	73.4	10.68	1882.12	43.1	9.45
1868.16	71.4	10.95	1883.14	39.6	9.11
1869.17	71.0	10.93	1884.22	36.8	8.60
1870.15	67.0	11.11	1885.19	33.8	8.04
1871.21	65.1	11.29	1886.14	29.7	7.40
1872.22	62.2	11.34	1887.19	24.4	6.79
1873.21	60.8	11.12	1888.24	23.3	5.78
1874.19	58.6	11.18	1888.97	13.9	5.27
1875.22	56.5	11.28	1890.27	359.7	4.19
1876.22	55.2	11.19			

These positions were plotted on a sufficiently large scale to lay them down accurately to the decimal places. The time covered by the measures was separated into four-year intervals by the lines shown on the diagram (plate 5), and after repeated trials a satisfactory ellipse was drawn which would make these areas equal with slight changes in the position-angles. This ellipse gives a period of 53 years.

This result may not be any nearer the true time than the periods found by Gore and Howard, as it is undoubtedly true that an ellipse with a greater major axis, and consequently a longer period, would represent the arc already described by the companion about as well as any other. With reference to this matter, I have been influenced by one consideration which could not have been taken into account in the previous attempts to compute the period. I measured *Sirius* last year (1890) with the 36-inch telescope, but in consequence of my absence on the Eclipse Expedition to Cayenne, and the severe winter weather that prevailed on Mount Hamilton for some time after my return, *Sirius* was long past meridian before I could get it even in early twilight; the companion was therefore very hard to see at all, and the measures were made with extreme difficulty.

This I attributed to the unfavourable conditions, and expected to have no trouble in seeing and measuring the small star this year. I have tried to get it this year under very favourable circumstances, when the most difficult and close pairs were comparatively easy, and have utterly failed to see the least trace of it; I am satisfied that it is now beyond the reach of any telescope in the world. The distance, according to my measures in 1890, was $4''.19$. It seems hardly possible that it could have been wholly invisible this year under the conditions named, if the diminution in the distance is only $0''.7$, as it should be according to the orbits of Gore and Howard. I have, therefore, concluded that the small star is approaching *Sirius* more rapidly, and that the change in distance must be decidedly larger, and certainly not less than $1''$, and consequently have drawn the shorter ellipse as more likely to represent the real motion. According to this, the distance at the time of my last attempts to see the companion was $3''.1$, and it is not at all improbable that, at this distance from a star as bright as *Sirius*, it would be lost in the overpowering brilliancy of the large star, even with the Lick telescope. For these reasons I think a period of 53 years is not likely to be too short.

If this ellipse is correct the minimum distance will be $2''.4$ in 1892-5, and about the end of 1894 the distance will be the same as at the time of my last measures. At that time, therefore, I trust the large telescope will supply observations which will definitely settle most of the uncertainties in the orbit of this interesting system.

Lick Observatory:
1891 February 26.

Invisible Double Stars. By S. W. Burnham.

In addition to a large number of double stars which have been seen and measured in the regular way, we have now a new class of binaries which may be designated as invisible double stars, since they have never been seen, and are only recognised from certain peculiarities in their appearance or motions. What is known of them is due to comparatively recent investigations. It can hardly be said that their existence has been proven in any instance, and the *primâ facie* case made is not always very strong. This might be expected when it is considered that the inference is deduced from observations more or less in error; and as the difficulty of eliminating any real change from errors of observation much larger in amount is very great, the result must be received with a good deal of allowance, and particularly so when the time embraced by the observations is limited.

The latest additions to this class of double stars are due to the spectroscope, which has shown a periodical doubling of

certain lines, which, it is inferred, must be caused by the alternate approach and recession of the components of a close double star in rapid motion. This method has both the advantages and the disadvantages of being entirely independent of former methods of observing. It is unaffected by the errors and imperfections of other observers; and the conclusions derived from it are without reference to what has been seen and done before; while on the other hand there has been as yet no confirmation of the correctness of these conclusions by the spectroscope or by the telescope. Of the revelations of this instrument in showing regular periodical changes in the spectrum there can be no doubt. Whatever there is of uncertainty is in the interpretation to be given to this phenomenon. At present the most probable, if not the only explanation is that this is due to the extraordinarily rapid revolution of two suns about each other. This may be the true theory, but it might be unwise or unsafe to claim at present that this recurrent phenomenon could not be due to some other cause. The wonders already accomplished by the spectroscope indicate its possibilities in the future, and the most advanced worker would hardly either limit its revelations hereafter, or say how much that is accepted now may not ultimately require some modification. It is always unsafe to rely too implicitly on any theory for the interpretation of observations which appears probable because no better can be suggested. Astronomy is perhaps at the head of the sciences in mistakes of this kind, although at the time these theories were brought forward, from the standpoint of knowledge as it then existed, the conclusions arrived at appeared to be sound and justifiable. Thus far the spectroscopic method of discovering new double stars does not seem to have been verified by applying it to stars known to be double, and, so far as appears, it has not been attempted. While there can be no hope of finding any known binaries with the remarkably short periods assigned to the late discoveries with the spectroscope, there must be many having sufficiently rapid relative motion to be entirely suitable to prove beyond all question that this is or is not the true explanation. A few trials of the close and short period binaries would probably settle at once whatever doubt there may be in this regard. Most of the known binaries are probably fainter than the stars discovered in this way, but that is not a very serious matter in photographing the spectrum, since the same result is reached by continuing the exposure sufficiently long. It is not necessary to go on photographing a pair in which this doubling of the lines is found. It is enough that it appears at all.

The other class of stars supposed to be double is due to apparent irregularities in their motions, as shown by meridian and micrometrical observations. With the single exception of *Sirius*, all the stars of this kind have remained visually single, or rather the supposed disturbing body has resisted all attempts to see it with the most powerful instruments. The results of the

most careful watching of these stars have been so uniformly negative that one is led to expect very little based upon evidence of this kind; and the double star discoverer would be justified in coming to the conclusion that the surest way of finding a single star is to look at one which, according to theoretical considerations, must certainly be double.

Too much reliance has been placed upon the infallibility of observations. It is always much more probable that the apparent irregularities of motion are due to the unavoidable errors of observation than that a star in the system sufficiently important to produce such a real effect should be at all times invisible.

The double star of this class which appears to stand upon the most substantial basis, and which has been most thoroughly investigated from what might be termed theoretical data, is ζ *Canceri*. From the irregularities in the motion of the more distant star around A B, it was inferred that it was a close pair, or attended by a fourth star; and Seeliger in an exhaustive analytical investigation has shown that the third star, C, with its invisible companion appear to rotate in about $17\frac{1}{2}$ years. Perhaps no double star in the heavens has been more systematically measured than ζ *Canceri*. For more than 60 years we have more or fewer measures every year. During this interval the known close pair, A and B, have completed more than one revolution; and around these stars C has moved through an arc of 35° . This triple received special attention at Pulkowa, and the observations of O Σ cover a period of 40 years. Dembowski also measured it for many years, and most of the leading double star observers have given it more or less time. Therefore, the material to work with for any purpose is as good as would be found relating to any pair. The peculiar grouping of the positions of C was noticed a long time ago in the Pulkowa measures, and the existence of some third disturbing body suspected. Then followed finally Seeliger's investigation previously referred to.

In examining a matter of this kind the first step is to lay down all the observations accurately to scale, so that the eye can take in at a glance the entire movement of the stars as shown by the measures. This cannot be done by an inspection of tabulated results in whatever shape they may be put. The second step is to ascertain whether the peculiarities of motion are found in other and similar pairs, because, if these apparently discordant positions are rather the rule than the exception, it would go far to prove that they were due to inaccurate measures, and not to any mysterious disturbing influence.

An examination of the measures of the star C of ζ *Canceri* shows that the positions of that star are not distributed uniformly, but grouped more or less regularly along the path of its motion. This has been taken to mean that from some disturbing cause, not heretofore recognised, the motion of the star is alternately accelerated and retarded; and as this could be produced in one way

by a fourth, unseen, star, it is therefore assumed that C is attended by a dark star, the mass of which is about the same as that of C.

Being curious to know just how far such an assumption would appear to be warranted by the actual observations, I have selected measures representing as nearly as possible every year since the first observation by Struve, and have laid them down on a large scale, shown in a reduced form in the accompanying diagram (plate 6). All of the years from 1826 to 1888 inclusive are represented except 1827, 1829, 1830, 1834, 1837, 1838, 1839, and 1841. Preference has been given to the measures of Struve and O. Struve, and, when these were not to be had, to the measures of Dembowski, Dunér, Dawes, and other experienced observers. The vertical scale on the left gives the position-angles, and the scale on the top the distances. The sequence of the measures is shown by the lines joining the successive places. The time-scale on the right is made by dividing the whole interval covered by the measures into equal parts.

It is unnecessary to refer to what is shown by this diagram. The whole history of this star, so far as the measures are concerned, can be seen at a glance. The arrangement of the observed positions is certainly suggestive of some variation; and there is no doubt that the three groups of positions occur at substantially equal intervals. The question whether this is due to errors of observation is not an easy one to solve, if we consider only these observations. To show the relation between the observed and theoretical positions of C, I have given the latter, as computed by Seeliger, by the side of the actual measures, and on the same scale.

The next step was to find out, if possible, if any such peculiar motion of the companion star was shown by the measures of any other pair. To make the case parallel, it was necessary to select pairs having slow motion in position-angle of 15° to 35° , and which had been frequently measured during the same or an equal period of time. There are stars of this kind which have about the same angular velocity as ζ *Canceri*, differing not enough in distance to sensibly change the errors of measurement, and which have been measured largely by the same observers who observed the pair in question. Of course these would all be old and familiar pairs, and necessarily among the easiest objects in the heavens to measure. Among the stars selected as suitable for this purpose are *Castor*, ϵ^2 *Lyræ*, ζ *Aquarii*, γ *Leonis*, ι *Leonis*, ξ *Scorpii*, and ϵ *Hydræ*. I have plotted the measures of each of these stars in the same way (plates 7 to 9), and on the same scale, as the measures of ζ *Canceri*, and they are therefore strictly comparable. It should be noted that none of these stars have been as frequently measured as ζ *Canceri*, and, therefore, any peculiarity in the arrangement of the positions would be less prominently marked.

We find in each of these stars not only the same tendency to a grouping of the positions, but in some of them they occur at

intervals quite as regular as those of ζ *Canceri*. Take *Castor*, for instance (for which we have fifty-one measures, 1819-1878), and we find, if these groups of measures are to be taken as real, that the companion may be supposed to be revolving round a dark star once in about eight years. The circles which I have drawn round the groups are $0''.5$ in diameter, and the years attached to them give the mean dates of the included observations.

In ζ *Aquarii* (1821-1871) we have similar discordances, and with a little imagination these variations might appear to recur periodically.

In ϵ^2 *Lyræ* (1819-1877) there is the same appearance, more or less regular, of accelerated and retarded motion, but only half of the years are represented by measures, and, therefore, the grouping of the measures is not so well shown.

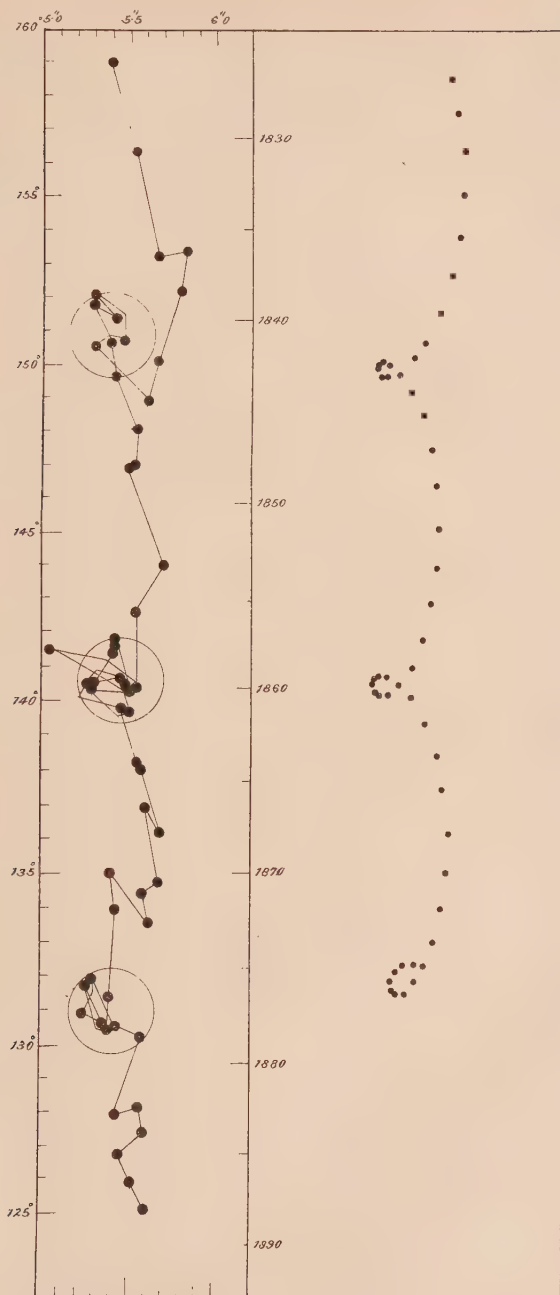
In ξ *Scorpii* there is one very strongly marked group, with few and scattering positions on each side. This is a triple star, similar in all respects to ζ *Canceri*, the third star in each case being measured from a close pair. About three-fourths of the years (1820-1877) are represented by measures.

In γ *Leonis* three distinct groups are fairly well marked at intervals of ten years, ending with 1872. The years for which there are no measures are all previous to 1845, with the single exception of 1849. The angular motion is less than that of most of the other pairs represented.

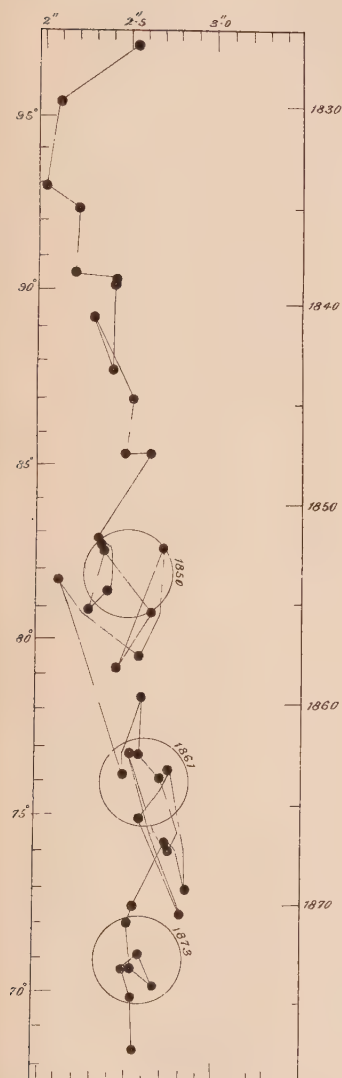
In ϵ *Hydræ* (1825-1889) we have three fairly well defined groups with an apparent period of thirteen years. There are twenty-one years not represented by measures, of which fifteen are previous to 1851. Eight of these missing years would come within the range of the 1844 period. It is well known that in this instance the principal star is a close pair, but it is much too close to have any influence whatever on the measures. The maximum distance probably does not exceed $0''.25$, and this, with unequal components, could not possibly affect the measures with any telescope. Even with the 36-inch it would not be recognised as a double star with any power that would ordinarily be used in measuring C. There is probably no doubt that Schiaparelli's pair is moving, and perhaps rapidly. My measures indicate a motion in angle of about 16° per annum.

It is evident that orbits could be computed for at least some of these stars which would satisfy the observations as well as has been done in the case of ζ *Canceri*, since it is only necessary to have the groups of measured positions occur at substantially regular intervals.

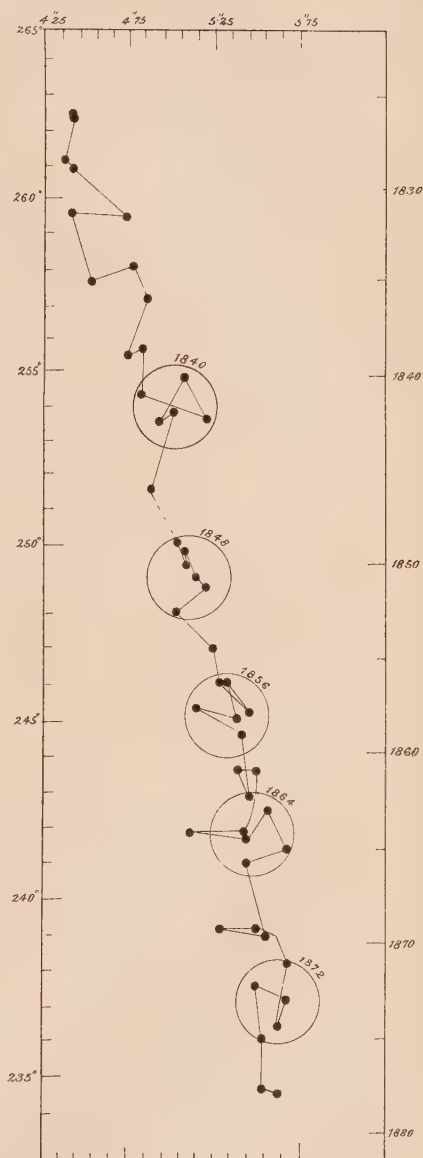
It should be stated that these apparent peculiarities of motion are not exceptional in stars of this class. I have given here the diagram of every star whose record I have investigated, with the single exception of ϵ *Equulei*, which is omitted because the whole angular change is only 10° , and this is but a little more than the average error of many of the measures. I am satisfied from these examples, and from looking over the measures of



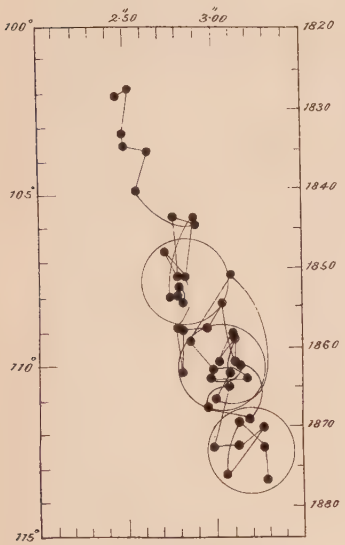
ζ CANCRI



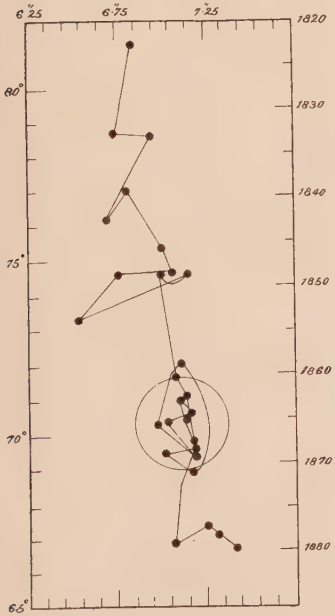
LEONIS.



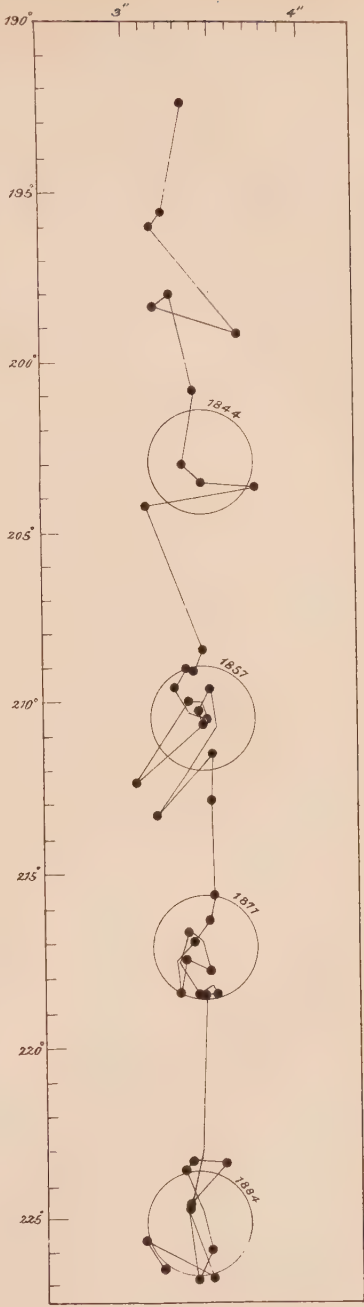
CASTOR.



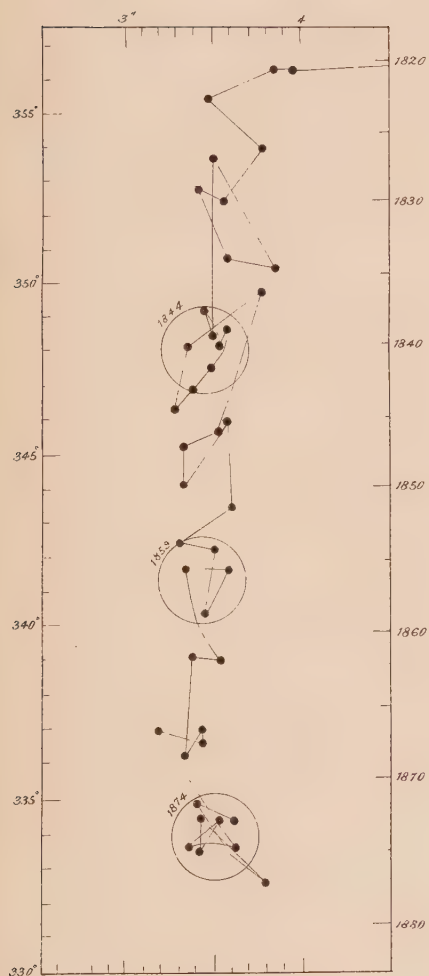
γ LEONIS



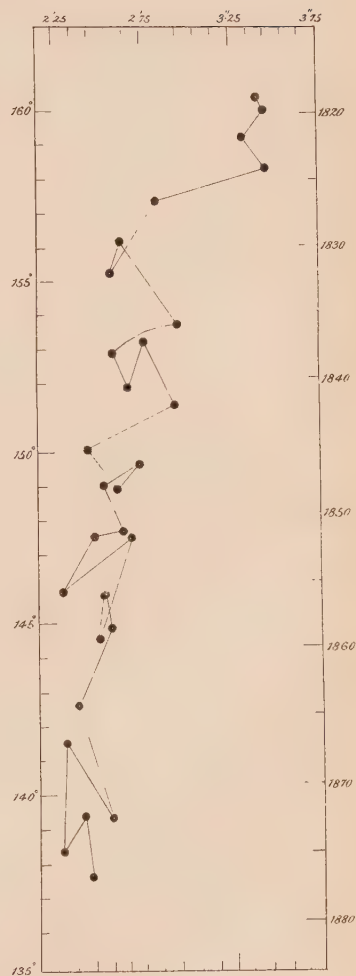
ξ SCORPII



ϵ HYDRAE



ζ AQUARI



ε LYRAE.

other stars of this kind, that similar discordances or peculiarities will be found in all. A partial explanation at least will readily suggest itself when it is remembered that all of these stars have a general movement of only about half a degree per year; and as the average error of the measures of angle is decidedly more than that, and in many instances several times that amount, it follows that apparent retrograde movements and groups of positions must be shown where there are annual measures. There is nothing extraordinary in such a case of the positions of even half a dozen years falling in about the same place; and that there is nothing wonderful in the crowding together of the positions at nearly equal intervals is demonstrated by the frequency with which it occurs.

It is unnecessary to discuss the question how far, if at all, ζ *Canceri* differs from the other pairs referred to. With the graphical representations of the several movements before him, each one can decide this matter for himself as intelligently as if he had canvassed and investigated the whole subject. It is not so much a matter for mathematical inquiry as for the exercise of sound judgment in considering whether any or all of these discordances are due to errors of observation.

The observer who has spent a good many years of his life in the discovery of new double stars, and particularly after finding new and more difficult components in scores of the old pairs, would be the last to say that a component of ζ *Canceri*, or of any other double star in the heavens, was not itself double. One never looks at an old pair without expecting to find a new star, and it is certainly as promising a place for discovery as any other. The most that any observer can say is that any given star, if double, is beyond the power of his telescope.

Leaving out of view for the moment the various stars mentioned as illustrations of apparent changes, and assuming that the variations of the star C of ζ *Canceri* are real, there is another, and perhaps a more natural explanation of variable motion, and that is that the centre of gravity of A and B is not in the middle of the line joining the two stars. It is infinitely more probable that two stars of about the same brightness have unequal masses than that a third nearly as bright star is balanced by one of the same mass which cannot be seen at all. Of course this would not produce a similar recurrence in point of time, but would help to explain the variations shown by the measures.

There is also a special possible source of error in the measures of C, due to the fact that in many of the observations A and B are treated as one star, and the measures made from the assumed middle of the double or elongated disc. Every practical worker with the micrometer knows how much uncertainty in both angles and distances there is in measures made under such circumstances. This is not a close pair, and even when the distance is minimum it would be called a comparatively easy object with

modern telescopes; and yet these stars at this time are more than 10° apart as seen from C. This would require the observer to guess at the middle point of an arc of 10° , which is constantly changing its angle with the bisecting line. That this alone would be the cause of considerable error there can be no doubt. It is not easy to bisect a line at right angles; it is still more difficult to bisect an oblique line. It may be worth while to point out as a fact, having possibly some significance in this connection, the relation of these lines at the times of the critical positions of C. The dates given are the mean epochs of the groups of C as actually measured.

	A B and C.	A and B.	
1842	15 ⁰	36 ⁰	29 ⁰
1859	141	290	31
1877	131	100	31

It will be seen that the line joining A B and C makes practically the same angle with A and B at each of these periods, or about 30° .

Another fact may be mentioned as a curious coincidence, if nothing more. The orbital motion of A B is in the same direction as the general motion of C around that pair. In 17 years C moves forward 10° , so that A B make exactly a quarter of a revolution with reference to the place of C during one rotation of the theoretical pair, and therefore the lines connecting the three stars have the same angular rotation for each period, and recur in the same order.

If the apparent change in C is not due to any fourth dark star, but is caused by errors of observation, augmented perhaps by some of the sources of error mentioned, it then follows that with a repetition of the same kind of observations we shall have the same grouping of positions at substantially the same intervals; and, therefore, such a recurrence will be no proof of the existence of a fourth star. In view of the actual measures of this star, taken in connection with the similar variations shown in the measures of other stars where there is no question of any disturbing influence, it seems certain that special observations will have to be made of this star, using every precaution to eliminate as far as possible all sources of error in order to determine, first, whether there is any real variation in its motion; and secondly, if there is, to what is it due? One method is to measure it from A and B separately, with instruments of sufficient separating power so that the other member of the closer pair shall affect the bisection of the one being measured from as little as possible. If measurements of this kind are not made with instruments of the best definition, and under the most favourable atmospheric conditions, it is quite certain that there will be an appreciable error in the distances, and particularly in the angles; and even then it may not be possible to get rid of

the effect of one star in measuring another star so near it. Obviously the best way, and the only independent one, would be to measure C from some entirely outside point.

The nearest star at all available for this purpose is D. M. +18°, 1870, a 9.1 magnitude star, which is 286'' from C in the direction of 108°.2. As this is nearly in the line joining A B and C (123°) it would be sufficient to measure the angle only, and this could be done with great accuracy; but the distance is so great, the resulting errors of observation might still be too large to determine whether the angular motion of C is uniform. However, some of the other pairs mentioned doubtless have convenient stars to be tested with the micrometer in this manner, for it can hardly be questioned that the same explanation will apply equally to all.

Perhaps the best independent method in the case of ζ *Cancri* would be to compare C with a neighbouring star, measuring annually the difference of declination with the meridian-circle. The changes in the place of C, according to theory, should be now, and for several years to come, principally in declination, and this should be shown by differential meridian observations covering a series of years. There is a 7.4 magnitude star 2^m following and about 1.5 north which would be suitable for this comparison. The position-micrometer could be used with this star to measure the difference in declination.

Lick Observatory:
1891 March 24.

A Comparison of the North Polar Distances of the Nautical Almanac for 1880 with the Cape Catalogue, the Greenwich Ten-year Catalogue, and Boss's Standard Star Places for 1880. By W. Grasett Thackeray.

The following comparison was made for the purpose of seeing whether the Greenwich Ten-year Catalogue Star Places for 1880 would corroborate the results of a comparison between the *Nautical Almanac* and Cape General Catalogue for 1880, given by Mr. Stone in a paper published in the *Monthly Notices*, xl. 2, pp. 57-70.

In this paper it was shown that the discordances in N.P.D. (Cape-Greenwich), arranged for every six hours of R.A., gave the following corrections after applying the general mean correction of -0''.31 :

0 ^h -6 ^h .	6 ^h -12 ^h .	12 ^h -18 ^h .	18 ^h -24 ^h .
-0''.24	-0''.11	+0''.39	+0''.06

and it was further remarked that the observations corresponding to the result grouped under 0^h-6^h were made during the dry

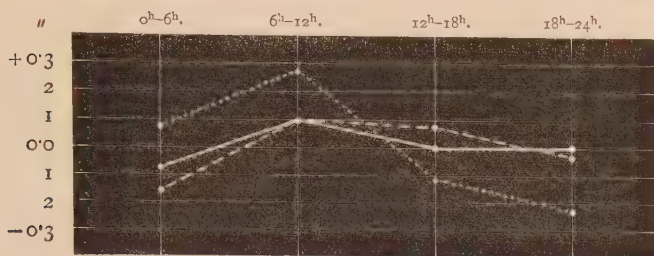
season at the Cape, and that the observations corresponding to the result grouped under 12^h-18^h were made during the wet season at the Cape. The conclusion, therefore, seemed to be that the discordances were systematic, and must be connected with the changes of temperature or moisture which take place at these different seasons at the Cape. From the present comparison it would appear that the discordances are not due to these causes.

- (1) The comparison between the *Nautical Almanac* and the Cape Catalogue for 1880 has been taken from Mr. Stone's paper, mentioned above.
- (2) The comparison between the Greenwich Ten-year Catalogue and the Cape Catalogue for 1880 depends only on the *Nautical Almanac* stars used in the above comparison. As the number of observations of each star in the Greenwich Catalogue is much greater than in the Cape Catalogue, the means were obtained by weighting the results according to the number of observations in the Cape Catalogue with a maximum weight of 10 to any one star; the results are found to be practically the same as if every star had had the same weight.
- (3) (4) (5) Boss's standard star places were obtained by applying to the *American Nautical Almanac* star places for 1880 the corrections derived from the Appendix, Table VIII., equal weights being given to every star.

	0^h-6^h .	6^h-12^h .	12^h-18^h .	18^h-24^h .	Mean.
(1) <i>N. A.</i> , 1880-Cape ...	+ "55	+ "42	- "08	+ "25	+ "31
(2) Greenwich-Cape ... (1880) (1880)	+ '36 ₄₃	+ '52 ₄₂	+ '31 ₃₅	+ '42 ₃₉	+ '40
(3) Greenwich-Boss ... (1880) (1880)	+ '30 ₂₃	+ '30 ₂₁	+ '20 ₂₀	+ '38 ₂₂	+ '30
(4) <i>N. A.</i> , 1880-Boss ... (1880)	+ '40 ₂₆	+ '27 ₂₁	- '12 ₂₀	+ '09 ₂₂	+ '16
(5) Cape, 1880-Boss ...	- '19 ₂₆	- '28 ₂₁	- '30 ₂₀	- '04 ₂₂	- '20

The subscribed figures denote the number of stars on which the results depend. Correcting each of these results for the mean error—

	0^h-6^h .	6^h-12^h .	12^h-18^h .	18^h-24^h .
Greenwich, 1880-Boss ...	"00	"00	- "10	+ "08
<i>N. A.</i> , 1880-Boss ...	+ '24	+ '11	- '28	- '07
Cape, 1880-Boss ...	+ '01	- '08	- '10	+ '16
Greenwich, 1880-Cape, 1880	- '04	+ '12	- '09	+ '02



The mean places of the stars in the *Nautical Almanac* for 1880 have been derived from the Greenwich Catalogues for 1860 and 1864, and for the stars not therein contained from the Cape Catalogue of 1,159 Stars, the first Melbourne General Catalogue, and the Cape Separate Catalogues 1871-3, and hence it would appear that the Greenwich Ten-year Catalogue places arranged in order of right ascension do not agree with those derived from the 1860 and 1864 Catalogues as given in the *N. A.*

The following comparison between the *N.A.* star places for 1880 and 1890 was made in order to compare the 1860, 1864, and 1872 Greenwich Catalogues together (the *N.A.* star places for 1890 depending on the 1872 Catalogue). The stars used in this comparison are those only which were used in Mr. Stone's paper, and the precession, secular variation, and proper motions have been taken from the Ten-year Catalogue.

The other two comparisons are obtained indirectly from the last and former comparisons.

	h h 0-6.	h h 6-12.	h h 12-18.	h h 18-24.	Mean.
<i>N. A.</i> , 1880— <i>N. A.</i> , 1890 ...	+ "13	"00	+ "18	+ "12	+ "11
Greenwich Cat., 1880— <i>N. A.</i> , 1880	— "19	+ "10	+ "39	+ "17	+ "12
Greenwich Cat., 1880— <i>N. A.</i> , 1890	— "06	+ "10	+ "57	+ "29	+ "22

From these results it would appear that the Mean North Polar distances of stars in the *Nautical Almanacs* for 1880 and 1890, which are derived from the Greenwich Catalogues for 1860, 1864, and 1872, are liable to a periodic error when arranged in order of right ascension, and that this error (probably largely due to the differences of the adopted proper motions) has been eliminated in the Ten-year Catalogue for 1880.

Approximate Proper Motions of some Groombridge Stars. By W. Grasett Thackeray.

The following Groombridge stars, which have been observed at Greenwich since 1876, and for which no proper motions are known to have been determined, were found to have proper motions either of at least $\cdot 01^s$ in right ascension or $0''\cdot 1$ in north polar distance. The proper motions have been provision-

ally determined by taking the places as given in the Radcliffe Catalogue for 1845 for the Groombridge and Radcliffe observations, and by reducing the Greenwich observations back to the year 1845 by applying the secular variation to the precessions given in the ten-year or annual catalogues.

The results, which are only meant to call attention to the fact that the star-places are liable to sensible errors when brought up from some years back, have been given both from the comparison of the Groombridge and Radcliffe places, and of the Radcliffe and Greenwich places, and are not sufficiently accurate to be appreciably affected by the difference between Bessel's and Struve's precessions. The results are in many cases more accordant than might have been expected:—

Groombridge Nos.	Proper Motion in Right Ascension.		Proper Motion in North Polar Distance.		Groombridge Nos.	Proper Motion in Right Ascension.		Proper Motion in North Polar Distance.	
	Groombridge and Radcliffe. s	Radcliffe and Greenwich. s	Groombridge and Radcliffe. "	Radcliffe and Greenwich. "		Groombridge and Radcliffe. s	Radcliffe and Greenwich. s	Groombridge and Radcliffe. "	Radcliffe and Greenwich. "
9	+ '01	+ '01	- '01	- '02	1775	- '01	...	- '11	- '12
11	+ '01	+ '01	+ '06	...	1781	...	...	+ '09	+ '11
30	+ '01	+ '01	+ '04	+ '04	1930	+ '02	+ '02	- '01	+ '01
145	+ '08	+ '01	+ '24	+ '25	1971	- '01	- '03	+ '02	- '04
356	+ '02	+ '02	+ '19	+ '24	1972	- '02	- '02	+ '04	- '04
628	+ '02	+ '01	+ '03	+ '01	2044	- '01	- '01	+ '06	+ '05
706	+ '02	+ '02	+ '16	+ '16	2089	...	...	+ '10	+ '13
717	- '01	- '01	+ '08	+ '16	2141	...	- '01	- '21	- '18
752	+ '01	+ '01	+ '13	+ '18	2196	+ '14	+ '09	+ '26	+ '23
757	...	- '01	- '08	- '13	2391	+ '05	+ '02	- '17	- '22
762	+ '06	+ '04	+ '28	+ '27	2527	+ '02	+ '01	- '21	- '29
828	...	...	+ '15	+ '09	2538	- '01	- '02	- '13	- '02
936	...	...	+ '11	+ '14	2571	- '02	- '03	- '21	- '23
1178	+ '02	+ '01	+ '24	+ '19	2580	+ '01	+ '01	- '12	- '19
1209	...	...	+ '10	+ '09	2867	+ '01	+ '01	- '32	- '33
1214	+ '01	...	+ '13	+ '19	3249	+ '01	...	+ '16	+ '26
1216	+ '02	+ '01	+ '25	+ '21	3263	...	...	- '19	- '19
1228	+ '02	+ '02	+ '03	+ '06	3295	+ '01	+ '02	+ '13	+ '17
1281	+ '01	...	+ '20	+ '20	3409	- '01	- '01	+ '17	+ '09
1400	+ '05	+ '03	- '11	- '07	3636	+ '02	+ '02	- '09	- '07
1482	+ '01	...	- '18	+ '17	3703	+ '02	+ '01	- '05	- '04
1517	- '01	- '02	+ '10	+ '05	3882	+ '02	+ '02	- '06	+ '01
1527	- '01	- '01	- '07	- '06	3904	+ '02	+ '02	- '12	- '07
1668	...	- '01	+ '13	+ '06	4177	...	- '01	- '24	- '24

The Perturbations of Sappho (80). By Robert Bryant, B.A., D.Sc.

The following values of the perturbations of *Sappho* form a continuation of those previously published by me. As previously, the elements have been corrected for the effect of perturbation every forty days. Great pressure of other matters has prevented me from preparing for the time of the opposition of this planet in 1889 an ephemeris such as will be required for the reduction of the observations for determining its parallax. I have partially revised my previous work on the orbit of this planet. Dr. Otto Knopf has pointed out to me errors in some of the normal places, in correcting some of which a better representation of the observations is obtained. I have also found minor mistakes in the calculation of the perturbations, which must be expected when one deals single-handed with three millions of figures; but these have no great effect upon the result. I have, however, detected an error of a more serious kind. In integrating the differentials from 1872 September 7, backwards, the element of time was regarded as positive, and the sign of the result was changed before applying the correction thus found to the elements, to obtain the effect of the terms depending upon the squares and higher powers of the masses of the disturbing bodies. The perturbation of the mean longitude consists of two parts, one of which is that arising from a change in the mean motion, and depends upon a double integral, and consequently the sign of the result in this case should have been left unchanged. Unfortunately the effect of this error of sign is such that (so far as this element is concerned) the result is less accurate than if the correction had been omitted altogether.

Disturbing Action of Venus.

1888.	ΔL_1 .	ΔL_2 .	$\Delta \pi$.	$\Delta \Omega$.	Δi .	$\Delta \phi$.	$40 \Delta \mu$.
Apr. 16	+0''77	+0''08	-3''14	+0''15	-0''10	-0''31	-0''176
May 6	0'00	0'00	0'00	0'00	0'00	0'00	0'000
26	-1'03	+0'05	+2'40	-0'06	+0'05	-0'13	+0'077
June 15	-2'06	+0'12	+3'15	-0'06	+0'05	-0'63	+0'036
July 5	-2'85	+0'08	+1'80	-0'06	-0'02	-1'33	-0'118
25	-3'17	-0'15	-1'48	-0'12	-0'12	-1'99	-0'355
Aug. 14	-2'96	-0'64	-5'85	-0'23	-0'22	-2'41	-0'625
Sept. 3	-2'23	-1'39	-10'11	-0'36	-0'30	-2'44	-0'865
23	-1'14	-2'34	-13'09	-0'41	-0'32	-2'11	-1'029
Oct. 13	+0'06	-3'41	-14'04	-0'34	-0'30	-1'56	-1'083
Nov. 2	+1'13	-4'47	-12'69	-0'10	-0'24	-0'97	-1'016
22	+1'89	-5'40	-9'31	+0'26	-0'16	-0'55	-0'833
Dec. 12	+2'21	-6'11	-4'74	+0'64	-0'10	-0'41	-0'569

	1889.	ΔL_1 .	ΔL_2 .	$\Delta \pi$.	$\Delta \Omega$.	Δi .	$\Delta \phi$.	40 $\Delta \mu$.
Jan.	1	+2 ⁰⁰ 9	-6 ⁰⁰ 53	-0 ⁰⁰ 19	+0 ⁰⁰ 91	-0 ⁰⁰ 06	-0 ⁰⁰ 61	-0 ⁰⁰ 275
	21	+1 ⁰⁰ 56	-6 ⁰⁰ 67	+3 ⁰⁰ 16	+0 ⁰⁰ 95	-0 ⁰⁰ 05	-1 ⁰⁰ 05	-0 ⁰⁰ 016
Feb.	10	+0 ⁰⁰ 77	-6 ⁰⁰ 59	+4 ⁰⁰ 43	+0 ⁰⁰ 71	-0 ⁰⁰ 07	-1 ⁰⁰ 58	+0 ⁰⁰ 150
Mar.	2	-0 ⁰⁰ 11	-6 ⁰⁰ 42	+3 ⁰⁰ 38	+0 ⁰⁰ 23	-0 ⁰⁰ 10	-1 ⁰⁰ 99	+0 ⁰⁰ 178
	22	-0 ⁰⁰ 87	-6 ⁰⁰ 29	+0 ⁰⁰ 44	-0 ⁰⁰ 34	-0 ⁰⁰ 13	-2 ⁰⁰ 10	+0 ⁰⁰ 057
Apr.	11	-1 ⁰⁰ 37	-6 ⁰⁰ 34	-3 ⁰⁰ 29	-0 ⁰⁰ 82	-0 ⁰⁰ 14	-1 ⁰⁰ 81	-0 ⁰⁰ 192
May	1	-1 ⁰⁰ 49	-6 ⁰⁰ 69	-6 ⁰⁰ 90	-1 ⁰⁰ 04	-0 ⁰⁰ 15	-1 ⁰⁰ 17	-0 ⁰⁰ 519
	21	-1 ⁰⁰ 20	-7 ⁰⁰ 38	-9 ⁰⁰ 04	-0 ⁰⁰ 94	-0 ⁰⁰ 15	-0 ⁰⁰ 32	-0 ⁰⁰ 854
June	10	-0 ⁰⁰ 52	-8 ⁰⁰ 38	-9 ⁰⁰ 22	-0 ⁰⁰ 64	-0 ⁰⁰ 16	+0 ⁰⁰ 51	-1 ⁰⁰ 131
	30	+0 ⁰⁰ 48	-9 ⁰⁰ 61	-7 ⁰⁰ 42	-0 ⁰⁰ 25	-0 ⁰⁰ 18	+1 ⁰⁰ 12	-1 ⁰⁰ 301
July	20	+1 ⁰⁰ 68	-10 ⁰⁰ 94	-4 ⁰⁰ 28	+0 ⁰⁰ 10	-0 ⁰⁰ 21	+1 ⁰⁰ 39	-1 ⁰⁰ 348
Aug.	9	+2 ⁰⁰ 81	-12 ⁰⁰ 26	-0 ⁰⁰ 93	+0 ⁰⁰ 34	-0 ⁰⁰ 24	+1 ⁰⁰ 24	-1 ⁰⁰ 268
	29	+3 ⁰⁰ 68	-13 ⁰⁰ 44	+1 ⁰⁰ 57	+0 ⁰⁰ 41	-0 ⁰⁰ 25	+0 ⁰⁰ 67	-1 ⁰⁰ 060
Sept.	18	+4 ⁰⁰ 20	-14 ⁰⁰ 35	+2 ⁰⁰ 52	+0 ⁰⁰ 31	-0 ⁰⁰ 22	-0 ⁰⁰ 21	-0 ⁰⁰ 749
Oct.	8	+4 ⁰⁰ 31	-14 ⁰⁰ 93	+1 ⁰⁰ 67	+0 ⁰⁰ 13	-0 ⁰⁰ 17	-1 ⁰⁰ 13	-0 ⁰⁰ 401
	28	+4 ⁰⁰ 03	-15 ⁰⁰ 16	-0 ⁰⁰ 66	-0 ⁰⁰ 03	-0 ⁰⁰ 09	-1 ⁰⁰ 86	-0 ⁰⁰ 089
Nov.	17	+3 ⁰⁰ 44	-15 ⁰⁰ 14	-3 ⁰⁰ 64	-0 ⁰⁰ 11	-0 ⁰⁰ 02	-2 ⁰⁰ 25	+0 ⁰⁰ 125
Dec.	7	+2 ⁰⁰ 64	-14 ⁰⁰ 96	-6 ⁰⁰ 16	-0 ⁰⁰ 13	+0 ⁰⁰ 02	-2 ⁰⁰ 22	+0 ⁰⁰ 200

Disturbing Action of the Earth.

	1888.	ΔL_1 .	ΔL_2 .	$\Delta \pi$.	$\Delta \Omega$.	Δi .	$\Delta \phi$.	40 $\Delta \mu$.
Feb.	16	-3 ⁰⁰ 36	-0 ⁰⁰ 11	+7 ⁰⁰ 37	-0 ⁰⁰ 23	+0 ⁰⁰ 08	-0 ⁰⁰ 20	+0 ⁰⁰ 176
Mar.	27	-1 ⁰⁰ 82	-0 ⁰⁰ 01	+3 ⁰⁰ 28	-0 ⁰⁰ 03	+0 ⁰⁰ 01	-0 ⁰⁰ 42	+0 ⁰⁰ 034
May	6	0 ⁰⁰ 00	0 ⁰⁰ 00	0 ⁰⁰ 00	0 ⁰⁰ 00	0 ⁰⁰ 00	0 ⁰⁰ 00	0 ⁰⁰ 000
June	15	+1 ⁰⁰ 47	+0 ⁰⁰ 04	-0 ⁰⁰ 83	-0 ⁰⁰ 03	+0 ⁰⁰ 04	+0 ⁰⁰ 79	+0 ⁰⁰ 090
July	25	+2 ⁰⁰ 34	+0 ⁰⁰ 23	+1 ⁰⁰ 40	0 ⁰⁰ 00	+0 ⁰⁰ 12	+1 ⁰⁰ 65	+0 ⁰⁰ 300
Sept.	3	+2 ⁰⁰ 55	+0 ⁰⁰ 67	+5 ⁰⁰ 87	+0 ⁰⁰ 12	+0 ⁰⁰ 21	+2 ⁰⁰ 24	+0 ⁰⁰ 588
Oct.	13	+2 ⁰⁰ 13	+1 ⁰⁰ 41	+10 ⁰⁰ 85	+0 ⁰⁰ 25	+0 ⁰⁰ 26	+2 ⁰⁰ 36	+0 ⁰⁰ 885
Nov.	22	+1 ⁰⁰ 25	+2 ⁰⁰ 41	+14 ⁰⁰ 26	+0 ⁰⁰ 23	+0 ⁰⁰ 26	+2 ⁰⁰ 00	+1 ⁰⁰ 106
1889.								
Jan.	1	+0 ⁰⁰ 17	+3 ⁰⁰ 56	+14 ⁰⁰ 52	-0 ⁰⁰ 04	+0 ⁰⁰ 21	+1 ⁰⁰ 46	+1 ⁰⁰ 177
Feb.	10	-0 ⁰⁰ 81	+4 ⁰⁰ 69	+11 ⁰⁰ 57	-0 ⁰⁰ 50	+0 ⁰⁰ 17	+1 ⁰⁰ 13	+1 ⁰⁰ 061
Mar.	22	-1 ⁰⁰ 36	+5 ⁰⁰ 61	+6 ⁰⁰ 97	-0 ⁰⁰ 91	+0 ⁰⁰ 14	+1 ⁰⁰ 31	+0 ⁰⁰ 783
May	1	-1 ⁰⁰ 44	+6 ⁰⁰ 22	+2 ⁰⁰ 94	-1 ⁰⁰ 04	+0 ⁰⁰ 14	+2 ⁰⁰ 00	+0 ⁰⁰ 417
June	10	-1 ⁰⁰ 01	+6 ⁰⁰ 45	+1 ⁰⁰ 22	-0 ⁰⁰ 89	+0 ⁰⁰ 13	+2 ⁰⁰ 96	+0 ⁰⁰ 056
July	20	-0 ⁰⁰ 09	+6 ⁰⁰ 37	+2 ⁰⁰ 58	-0 ⁰⁰ 63	+0 ⁰⁰ 11	+3 ⁰⁰ 77	-0 ⁰⁰ 205
Aug.	29	+1 ⁰⁰ 41	+6 ⁰⁰ 12	+6 ⁰⁰ 74	-0 ⁰⁰ 52	+0 ⁰⁰ 10	+4 ⁰⁰ 05	-0 ⁰⁰ 290
Oct.	8	+3 ⁰⁰ 61	+5 ⁰⁰ 85	+12 ⁰⁰ 78	-0 ⁰⁰ 58	+0 ⁰⁰ 12	+3 ⁰⁰ 69	-0 ⁰⁰ 227
Nov.	17	+5 ⁰⁰ 84	+5 ⁰⁰ 66	+18 ⁰⁰ 35	-0 ⁰⁰ 61	+0 ⁰⁰ 12	+3 ⁰⁰ 09	-0 ⁰⁰ 171
Dec.	27	+7 ⁰⁰ 33	+5 ⁰⁰ 59	+20 ⁰⁰ 11	-0 ⁰⁰ 60	+0 ⁰⁰ 15	+2 ⁰⁰ 22	+0 ⁰⁰ 024

Disturbing Action of Mars.

	ΔL_1 .	ΔL_2 .	$\Delta \pi$.	$\Delta \Omega$.	Δi .	$\Delta \phi$.	$40 \Delta \mu$.
1888.							
Feb. 16	-0°37	-0°01	+0°69	+0°02	-0°01	-0°08	+0°004
Mar. 27	-0°19	0°00	+0°38	+0°01	0°00	-0°04	+0°005
May 6	0°00	0°00	0°00	0°00	0°00	0°00	0°000
June 15	+0°18	-0°01	-0°36	0°00	0°00	+0°03	-0°010
July 25	+0°31	-0°02	-0°63	0°00	0°00	+0°07	-0°018
Sept. 3	+0°42	-0°04	-0°73	0°00	0°00	+0°12	-0°021
Oct. 13	+0°50	-0°06	-0°68	+0°01	0°00	+0°16	-0°017
Nov. 22	+0°56	-0°07	-0°49	+0°03	+0°01	+0°20	-0°007
1889.							
Jan. 1	+0°60	-0°07	-0°23	+0°05	+0°01	+0°21	+0°007
Feb. 10	+0°62	-0°06	+0°06	+0°06	+0°01	+0°21	+0°025
Mar. 22	+0°63	-0°02	+0°31	+0°07	+0°01	+0°18	+0°043
May 1	+0°63	+0°03	+0°49	+0°07	+0°01	+0°15	+0°061
June 10	+0°62	+0°10	+0°60	+0°05	+0°01	+0°11	+0°077
July 20	+0°61	+0°18	+0°64	+0°04	+0°01	+0°07	+0°092
Aug. 29	+0°60	+0°28	+0°63	+0°02	+0°01	+0°03	+0°106
Oct. 8	+0°59	+0°39	+0°56	+0°01	+0°02	0°00	+0°119
Nov. 17	+0°57	+0°52	+0°45	0°00	+0°02	-0°03	+0°131
Dec. 27	+0°56	+0°66	+0°31	0°00	+0°02	-0°05	+0°142

Disturbing Action of Jupiter.

	ΔL_1 .	ΔL_2 .	$\Delta \pi$.	$\Delta \Omega$.	Δi .	$\Delta \phi$.	$40 \Delta \mu$.
1888.							
Feb. 16	+ 7°42	- 9°79	+ 76°39	- 2°57	+ 1°23	+ 35°52	+ 9°461
Mar. 27	+ 6°24	- 2°52	+ 42°92	- 1°32	+ 0°77	+ 18°63	+ 4°993
May 6	0°00	0°00	0°00	0°00	0°00	0°00	0°000
June 15	- 12°43	- 2°61	- 50°57	+ 0°85	- 1°18	- 19°66	- 5°211
July 25	- 31°70	- 10°34	- 103°80	+ 0°37	- 2°82	- 39°36	- 10°122
Sept. 3	- 57°38	- 22°49	- 150°87	- 2°44	- 4°89	- 58°17	- 14°003
Oct. 13	- 87°36	- 37°68	- 180°57	- 8°43	- 7°20	- 75°28	- 16°034
Nov. 22	- 117°86	- 53°75	- 183°85	- 17°66	- 9°45	- 90°60	- 15°619
1889.							
Jan. 1	- 144°39	- 68°11	- 159°41	- 29°10	- 11°31	- 104°59	- 12°752
Feb. 10	- 163°67	- 78°65	- 115°60	- 40°89	- 12°56	- 117°80	- 8°127
Mar. 22	- 174°66	- 84°11	- 66°10	- 51°16	- 13°18	- 130°19	- 2°861
May 1	- 178°47	- 84°53	- 22°87	- 58°76	- 13°34	- 141°01	+ 1°954
June 10	- 177°32	- 80°66	+ 8°46	- 63°43	- 13°24	- 149°25	+ 5°574
July 20	- 173°53	- 73°79	+ 28°75	- 65°63	- 13°10	- 154°18	+ 7°673
Aug. 29	- 168°95	- 65°82	+ 42°85	- 66°13	- 13°04	- 155°64	+ 8°267
Oct. 8	- 164°90	- 57°78	+ 56°21	- 65°80	- 13°13	- 154°27	+ 7°632
Nov. 17	- 162°18	- 50°34	+ 72°45	- 65°40	- 13°39	- 151°01	+ 6°136
Dec. 27	- 161°20	- 45°68	+ 92°71	- 65°54	- 13°78	- 146°92	+ 4°152

Disturbing Action of Saturn.

1888.	ΔL_1 .	ΔL_2 .	$\Delta \pi$.	$\Delta \Omega$.	Δi .	$\Delta \phi$.	40 $\Delta \mu$.
Feb. 16	-1''00	-0''08	+ 1''29	+ 0''08	-0''03	-0''48	-0''057
Mar. 27	-0'50	-0'01	+ 0'74	+ 0'01	-0'01	-0'18	-0'008
May 6	0'00	0'00	0'00	0'00	0'00	0'00	0'000
June 15	+0'45	-0'01	- 1'00	0'00	-0'01	+0'08	-0'030
July 25	+0'83	-0'07	- 2'32	0'00	-0'02	+0'09	-0'094
Sept. 3	+1'09	-0'21	- 3'93	-0'04	-0'05	+0'05	-0'185
Oct. 13	+1'23	-0'45	- 5'80	-0'12	-0'08	0'00	-0'295
Nov. 22	+1'24	-0'80	- 7'82	-0'26	-0'11	-0'04	-0'415
1889.							
Jan. 1	+1'12	-1'28	- 9'86	-0'45	-0'15	-0'07	-0'535
Feb. 10	+0'88	-1'87	-11'76	-0'70	-0'17	-0'08	-0'642
Mar. 22	+0'56	-2'55	-13'40	-0'99	-0'19	-0'09	-0'723
May 1	+0'19	-3'30	-14'70	-1'29	-0'20	-0'14	-0'768
June 10	-0'20	-4'07	-15'67	-1'57	-0'19	-0'24	-0'766
July 20	-0'55	-4'81	-16'42	-1'82	-0'17	-0'42	-0'716
Aug. 29	-0'82	-5'48	-17'11	-2'01	-0'15	-0'69	-0'623
Oct. 8	-0'97	-6'05	-17'86	-2'13	-0'12	-1'00	-0'505
Nov. 17	-0'98	-6'49	-18'63	-2'18	-0'09	-1'30	-0'382
Dec. 27	-0'87	-6'83	-19'26	-2'17	-0'07	-1'56	-0'281

The above values are for Greenwich mean noon.

Reduction of Measures of the Photographs of Jupiter taken at the Lick Observatory in 1890. By A. Stanley Williams.

Through the kindness of Professor E. S. Holden, I have recently been favoured with particulars of an important series of measurements of the belts of *Jupiter*, made by him on the remarkably fine series of photographs of the planet which he obtained last year by means of the great Lick refractor. These measures furnished on reduction the zenographical latitudes of the principal belts; and, as the results are interesting in more ways than one, I herewith beg to present them to the Society.

The photographs measured were enlarged directly in the telescope 8.3 times, the image of the planet on the plates taken at about the time of opposition being very nearly an inch in diameter. The distances were measured by Professor Holden in millimetres from the north pole to the middle points of the chief belts, and the minor axis of the disc was also measured on each plate. In order to show clearly the method of reduction em-

ployed in the present case, the details are given fully below for one of the belts—namely, the dark belt No. 6 :

Date.	G.M.T. of Photograph.		Minor Axis.	Dist. from N. Pole.	Dist. from Centre of Disc.	Corresponding Latitude.	Correction D.	True Zenographical Latitude.
1890.	h	m	mm.	mm.				
July 8	21	3	21.5	16.0	-.488	-27°64	-°27	-27°91
13	21	13	21.6	16.0	.482	27.27	.28	27.55
Aug. 31	18	25	21.2	15.5	.462	26.01	.38	26.39
Sept. 1	18	3½	21.1	15.5	.469	26.45	.38	26.83
8	18	9	20.7	15.5	.498	28.28	.40	28.68
14	17	14	20.3	15 +	(.478)	27.01	.41	(27.42)
Oct. 19	15	17	18.4	13.9	.511	29.11	.42	29.53
19	15	29	...	13.7	.489	27.71	.42	28.13
27	15	10	18.0	13.0	-.444	-24.90	-.41	-25.31

The first four columns of the above table sufficiently explain themselves. The fifth gives the distance from the centre of the disc, where the semi-axis minor = 1; and the sixth the corresponding latitude according to Dr. Lohse's useful little table, published in "Publicationen des Astrophysikalischen Observatoriums zu Potsdam," No. 9, p. 7. Column 7 contains the corrections to be added to the preceding latitudes, arising from the elevation of the Earth above the plane of *Jupiter's* equator. They are taken from Mr. Marth's invaluable "Ephemeris for Physical Observations of *Jupiter*" in the *Monthly Notices*. Finally, in the last column are the true zenographical latitudes. The mean position of the middle of the belt No. 6, from the measurements of eight plates, is $-27^{\circ}54$, with a probable error of $\pm 0^{\circ}32$. The observation of September 14 is rejected for obvious reasons.

The measures relating to the other belts have been reduced in an exactly similar manner to the above. With one exception, where it is doubtful what was the exact part of the belt measured, all the measures have been made use of, though in some cases, by rejecting one or two of them, the separate results might be made a good deal more accordant and the probable errors of the final determinations smaller, the latitudes of some of the belts evidently varying a little in different parts of the planet. Belt No. 6 may be instanced as an example of this. The last measure (October 27) relates to the part just preceding the red spot, and the belt here was at that time clearly more northerly than at other parts.* A measure of the first plate of October 19, giving a latitude of $-17^{\circ}68$, has not been made use of, the middle of

* This belt was also considerably broader at some parts of the planet than at others, which circumstance must doubtless have affected the measures.

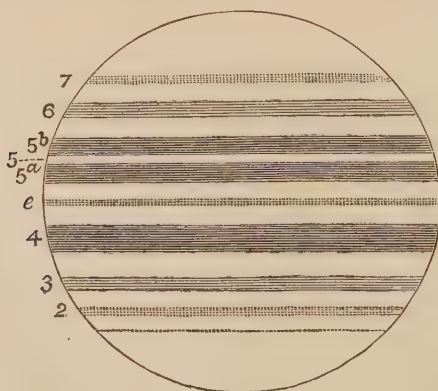
the bright zone south of the dark south equatorial belt having evidently been inadvertently measured, instead of the middle of the latter belt. The final resulting positions of the middle points of the principal belts are given in the following table :—

Belt.	Mean Latitude.	Dist. from Centra.	No. of Plates.
7	$-38^{\circ}45$	$-.647$	1
6	$-27^{\circ}54 \pm 0^{\circ}32$	$-.486$	8
5 ^b	$-15^{\circ}45 \pm 0^{\circ}07$	$-.283$	3
5	$-11^{\circ}80 \pm 0^{\circ}19$	$-.218$	11
5 ^a	$-8^{\circ}23 \pm 0^{\circ}24$	$-.153$	5
e	$+0^{\circ}21 \pm 0^{\circ}10$	$+.004$	18
4	$+10^{\circ}37 \pm 0^{\circ}17$	$+.192$	18
3	$+24^{\circ}55 \pm 0^{\circ}12$	$+.438$	18
2	$+33^{\circ}35 \pm 0^{\circ}55$	$+.575$	3

Red Spot.

S. edge...	...	$-28^{\circ}00$	$-.494$	1
Centre (apparent)	...	$-20^{\circ}53$	$-.371$	1
N. edge	..	$-13^{\circ}51$	$-.249$	1

The distances from the centre are those corresponding to the latitudes given in the second column, or when the elevation of the Earth above the plane of *Jupiter's* equator is nil. Belt No. 5 is the south equatorial belt. It is actually double, being composed of two parallel dark bands, separated by a narrow bright rift, and this duplex character is shown very clearly on several of the Lick photographs. 5^a refers to the northern and 5^b to the southern component of this double belt. By means of the distances from the centre given above, the positions of the belts have been laid down in the diagram.



On one plate, dated September 21, the outside dimensions of the red spot were measured by Professor Holden as follows: Length, 6.6^{mm} ; breadth, 2.4^{mm} ; distance from E. end to limb, 6.0^{mm} ; distance from W. end to limb, 6.3^{mm} . According to these measures the photograph was obtained 1.6^{m} before the centre of the red spot had arrived on the central meridian of the disc. The photograph was taken at either $16^{\text{h}} 8^{\text{m}}$ or $16^{\text{h}} 10^{\text{m}}$ G.M.T. Assuming the mean of these to be correct, the spot would have transited at $16^{\text{h}} 10.6^{\text{m}}$ G.M.T.; and the centre consequently preceded the zero meridian of Mr. Marth's ephemeris (System II.) by 8.2^{m} . Visual observations make the difference somewhat greater. From observations by the writer the spot at that time preceded the zero meridian by about 15^{m} .

Measures of the position of a small dark spot on the north side of the N. equatorial belt were also made on two photographs taken on July 13. These give for the time of the transit of the spot over the central meridian of the disc, $21^{\text{h}} 24.7^{\text{m}}$ G.M.T. from one plate, and $21^{\text{h}} 28.1^{\text{m}}$ from the other. The mean of these two determinations, $21^{\text{h}} 26.4^{\text{m}}$, agrees well with visual observations made about the same time, as will be seen from the following list of transits:—

	G.M.T. of Transit.		Foll. Mr. Marth's Zero Mer. System II. by		
	h	m	h	m	
1890.					
June 26	12	35.3	2	52.7	Williams, visual
July 3	13	16.3	2	48.7	" "
13	21	26.4	2	49.1	Holden, photo.
22	13	48.2	2	43.7	Williams, visual
Aug. 1	12	4.0	2	45.3	" "
2	17	49.5	2	44.1	Barnard*, visual
4	9	31.7	2	43.9	Williams, visual

As there are many measurable dark and light spots shown on the Lick photographs of *Jupiter* in the possession of the Society, suitable measures of this magnificent collection should form a valuable addition to the results obtained directly at the telescope.

It would be interesting to compare the positions of the belts given in this paper with corresponding results obtained by micro-metrical measures at the telescope, particularly with the view of ascertaining the relative degree of accuracy of the two methods. Unfortunately, however, it is seldom possible to make a satisfactory comparison, since, although many series of micro-meter measures have been made, the results have rarely been fully reduced. The points measured also are not always the same, some observers having measured the edges of the principal belts, and others the middle points of the bands, whilst in some cases it does not clearly appear what was the part measured.

* *Publications of the Astronomical Society of the Pacific*, vol. ii. p. 248.

Belt No. 3 was measured by Dr. O. Lohse, at Potsdam, on four nights in 1879. These measures I have reduced in the same manner as Belt No. 6, *supra*. The same belt was also measured by me in 1888 on three nights, and in 1890 on one night, so that we have the following results :—

Latitude of Belt No. 3.

1879	+ 20° 32' ± 0° 19'	Lohse (4 nights), micrometric
1888	+ 25° 36' ± 0° 38'	Williams (3 nights), micrometric
1890	+ 24° 55' ± 0° 12'	Holden (18 plates), photographic
1890	+ 24° 52'	Williams (1 night), micrometric

These measures show that a very considerable displacement must have occurred in the place of this belt. From the measures of the position of the red spot made by him on nine nights in 1879-81, Dr. Lohse found the mean latitude to be $-21^{\circ}52'$, with a probable error of $\pm 0^{\circ}25'$ *. The latitude being approximately the same as that of Belt 3, this is fairly comparable with the others, so far as regards the probable accuracy of the result.

It would appear from consideration of the probable errors in the above cases, and also from comparison of the agreement *inter se* between the individual results of Dr. Lohse's measures, and between the separate determinations of Professor Holden, that measures of photographs of *Jupiter*, in their present state of perfection, are equal in point of accuracy to the best micrometrical measures; whilst not the least of the advantages of the former method is the power of being able to make the measures at one's leisure, and to repeat them if necessary, or to supplement them by measures of fresh points, so as to be able strictly to compare the positions of objects with the results obtained in previous years.

* *Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, No. 9, p. 18. No correction seems to have been applied for elevation of the Earth above the plane of *Jupiter's* equator, so that the probable error is here probably larger than it would otherwise be. As this correction may exceed 3° , it is very important that it should not be neglected, or otherwise the results of one year may differ by more than 6° from those of another year. 6° of zeno-graphical latitude near the equator is equivalent to more than $2''\cdot2$ of arc, when the apparent diameter of the planet is $40''$.

Catalogue of 918 Orbits of Meteor Streams from the Observations of Mr. W. F. Denning. By J. Kleiber.

(Abstract.)

The following pages contain a catalogue of orbits of meteor-showers,* corresponding to the 918 radiant points given by Mr. W. F. Denning in the May number of the *Monthly Notices* for 1890.

The 918 radiants or orbits represent in reality a much smaller number of distinct streams, many streams being represented by more than one radiant. But as the identification of radiants is a matter still subject to controversy, I found it better to compute the elements for each radiant separately, giving thus a new criterion for their identification *à posteriori* by comparing the elements of their orbits.

The computation has been effected by means of special tables constructed *ad hoc*, which permitted to find directly all the elements *without neglecting the eccentricity of the Earth's orbit*. I retained in the course of the calculation tenths of a degree, rejecting these only in the final result, where I put $\frac{1}{2}$ for 0.3, 0.4, 0.5, 0.6, 0.7, and 1 for 0.8, 0.9, 1.0, 1.1, 1.2.

I may add, as a first preliminary result of my computation, that it confirms the shifting of the radiant of the *Perseids* during the six weeks of the activity of the shower, as observed by Mr. W. F. Denning. A glance at the orbits corresponding to all the radiants related by Mr. Denning to the *Perseid* shower, will show their identity at once.

In the catalogue the number is that of Mr. Denning's list: l, b denote the longitude and latitude of the radiant respectively, $\Omega, i, \pi - \Omega$, and q are the usual parabolic elements of the orbit corresponding to the radiant.

St. Petersburg :

1891 January 12.

Sur la Détermination récente de la Longitude Paris-Greenwich.

Par MM. Bassot et Defforges.

(Communicated by H. H. Turner, M.A., B.Sc.)

Voulez-vous nous permettre d'ajouter quelques considérations à celles qu'a déjà développées devant la société monsieur H.-H. Turner sur les résultats de la mesure de la différence de longitude entre Paris et Greenwich, en 1888?

Il existe en effet entre monsieur Turner et nous mêmes quelques différences d'appréciation, et comme il nous a très-loyalement priés de rendre notre opinion publique et gracieusement offert de la présenter à la Société Royale Astronomique,

* Extracted from a book now in the press, *On the Determination of Orbits of Meteor Showers* (in Russian).

nous vous demandons de vouloir bien accueillir ces quelques réflexions qui vous sont adressées par son intermédiaire.

Les valeurs individuelles obtenues pour la différence de longitude entre Paris (méridien de Cassini) et Greenwich (méridien du Transit-Circle) sont contenues dans le tableau suivant.

TABLEAU I.

Valeurs individuelles de la Différence de Longitude entre Paris et Greenwich.

Dates.	Echanges H T et L.		Echanges B et D.		Echanges H T et D.		Echanges B et L.		
		9 ^m s		9 ^m s		9 ^m s		9 ^m s	
Sept. 30	20	73	20	85	21	10	20	47	
Oct. 1	20	77	20	89	21	12	20	56	
3	20	98	20	99	21	19	20	72	
5	20	97	20	91	21	14	20	73	
11	20	65	21	17	20	81	21	05	
13	20	69	21	18	20	64	21	24	
14	20	72	21	25	20	71	21	26	
15	„	„	„	„	20	88	„	„	
17	20	73	21	20	20	84	21	14	
19	20	54	21	18	20	82	20	96	
20	20	60	21	22	20	82	20	98	
21	20	62	21	10	20	72	21	02	
23	21	30	20	97	21	32	20	96	
26	21	07	20	84	21	07	20	72	
27	21	19	„	„	„	„	„	„	
30	21	12	20	95	21	23	20	89	
Nov. 9	21	18	21	00	21	42	„	„	
13	21	02	20	84	21	16	20	67	
Moyennes	1 ^{re} période	... 20	86	20	91	21	14	20	62
	2 ^e „	... 20	65	21	19	20	78	21	09
	3 ^e „	... 21	15	20	92	21	24	20	81
1 ^{re} et 2 ^e périodes		... 20	76	21	05	20	96	20	86
2 ^e et 3 ^e „		... 20	90	21	06	21	01	20	95
Longitude moyenne		... 20	83	21	06	20	99	20	91

La comparaison des résultats moyens des 3 périodes fournit pour les valeurs des équations personnelles :

	H T-L. s	B-D. s	H T-D. s	B-L. s
1 ^{er} Echange d'observateurs	-0.10	+0.14	-0.18	+0.23
2 ^e „ „	-0.25	+0.14	-0.23	+0.14
Moyennes ...	-0.18	+0.14	-0.21	+0.19

L'équation personnelle B—D est restée remarquablement constante ; elle est d'ailleurs identique à la moyenne des valeurs déterminées, presque chaque année, depuis 1877, c'est-à-dire depuis 14 ans, entre les deux observateurs B et D.

Mais les différences des deux valeurs de chacune des équations B—L et HT—L semblent indiquer chez l'observateur L une variation d'équation personnelle assez notable.

La différence des équations personnelles moyennes deux à deux donne :

$$\begin{aligned} B-H T &= +0.37^s \\ B-H T &= +0.35 \end{aligned}$$

L'accord de ces 2 valeurs montre que l'influence des erreurs systématiques des quatre chronographes pendant les échanges est très petite et peut être négligée. Les divergences des valeurs trouvées pour la longitude n'en peuvent donc provenir.

Dans le but d'en rechercher l'origine, on a fait intervenir l'observateur "Standard" de Greenwich (A D). La pendule "Standard" de l'observatoire de Greenwich enregistre, par l'intermédiaire d'un relai distributeur, intercalé dans le circuit de l'observatoire, la seconde sidérale sur les deux chronographes de la longitude. La correction de cette pendule, donnée par A D, a été comparée chaque jour aux corrections trouvées par les deux observateurs de Greenwich.

On a obtenu, par cette comparaison, les nombres suivants :

TABLEAU II.

Dates.	A D—H T.	A D—B.	A D—L.	A D—D.
	^s	^s	^s	^s
Sept. 30	...	...	—0.16	+0.23
Oct. 1	...	...	0	+0.37
3	...	...	+0.08	+0.32
5	...	...	+0.24	+0.42
11	—0.12	+0.29	...	...
13	—0.23	+0.37	...	...
14	—0.07	+0.45	...	...
15	+0.03	"	...	...
17	—0.04	+0.33	...	...
20	—0.05	+0.31	...	...
21	+0.04	+0.44	..	...
23	...	...	+0.49	+0.47
26	...	...	—0.07	0
27	...	...	+0.15	"
30	...	...	+0.38	+0.46
Nov. 9	...	..	+0.12	+0.42
13	...	...	+0.11	+0.26
Moyennes...	—0.06	+0.37	+0.13	+0.33
				3 11

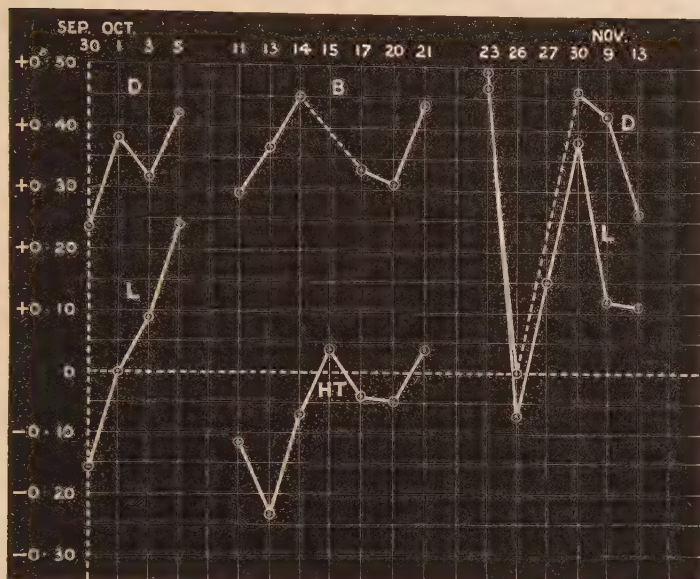
Prenant les moyennes de ces nombres pour chaque observateur comme la mesure de son équation personnelle avec A D, on a calculé au moyen des échanges de signaux enregistrés sur les chronographes la valeur de la longitude par la comparaison des heures observées d'une part à Greenwich par A D, et d'autre part à Paris successivement par H T, L, B et D. On a trouvé :

TABLEAU III.

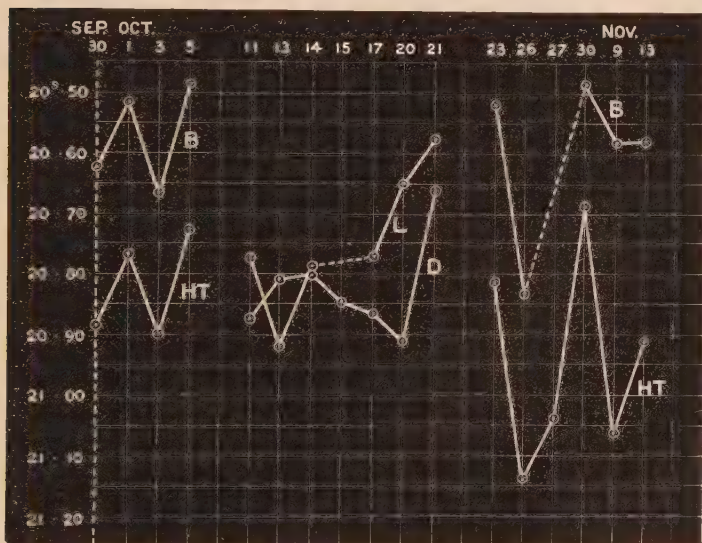
Dates.	Longitudes déterminées par			
	H T et A D.	B et A D.	L et A D.	D et A D.
Sept. 30	20°89	20°62	...	...
Oct. 1	20°77	20°52	...	...
3	20°90	20°67	...	...
5	20°73	20°49	...	...
11	...	...	20°77	20°88
13	...	...	20°92	20°81
14	...	...	20°79	20°80
15	...	...	„	20°85
17	...	...	20°77	20°87
20	...	...	20°65	20°91
21	...	...	20°58	20°66
23	20°81	20°52	...	...
26	21°14	20°84	...	...
27	21°04	„	...	...
30	20°74	20°49	...	...
Nov. 9	21°06	20°58	...	...
13	20°91	20°58	...	...
Moyennes	...	...	20°90	20°83
Equations personnelles	- 0°06	+ 0°37	+ 0°13	+ 0°33
Longitude conclue	...	20°84	21°96	21°16

C'est ainsi que H T et L paraissent s'accorder entre eux et que D et B semblent diverger de 0°2.

En examinant de plus près les deux tableaux précédents, nous avons été conduits à construire les diagrammes représentant d'une part les équations personnelles, calculées comme ci-dessus, des observateurs de Greenwich avec A D, de l'autre les valeurs qui en sont déduites pour la longitude. Ces diagrammes sont figurés ci-contre.



Courbes des Equations personnelles avec A D (Standard Observer).



Longitudes (Standard Observer).

Ils affectent une allure remarquablement parallèle, et les quatre ordonnées présentent à la fois les mêmes variations

systématiques, variations qui atteignent jusqu'à 0^s.55 d'un jour à l'autre.

Il n'y a, à notre avis, que deux manières d'expliquer ces variations.

Ou bien elles proviennent des variations mêmes de l'observateur AD, ce qui n'est admissible pour personne; ou bien elles proviennent de la variation journalière accidentelle d'un organe intercalé entre la pendule de Greenwich et les chronographes, et cet organe n'est autre que le relai distributeur qu'il a fallu régler chaque jour et dont l'influence a par suite chaque jour été différente, comme on s'en convainc facilement d'ailleurs en examinant les variations inadmissibles des nombres contenus dans les tableaux II et III.

Cela étant, le calcul précédent n'est plus légitime, car l'équation personnelle moyenne déterminée par rapport à AD pour un observateur, pendant son séjour à Greenwich, renferme un élément variable indépendant de l'observateur, et ne peut lui convenir lorsqu'il s'est transporté à Paris.

L'équation personnelle réelle de AD avec l'observateur de Paris (HT, B, L, D) peut être cependant calculée pour chaque jour en ajoutant à l'équation du jour avec AD de l'observateur de Greenwich (L, D, HT, B) l'équation personnelle HT-L, B-D, B-L, HT-D.

Comparant de cette façon l'heure déterminée par AD à Greenwich avec l'heure déterminée à Paris, on trouverait :

TABLEAU IV.

Dates.	Longitudes déterminées avec A. D.			
	par H T.	par B.	par L.	par D.
Sept. 30	20 ^s .55	20 ^s .99	...	...
Oct. 1	20 ^s .59	20 ^s .03	...	...
3	20 ^s .80	21 ^s .13	...	...
5	20 ^s .79	21 ^s .05	...	...
Moyennes	20 ^s .68	21 ^s .05	...	...
11	...	...	20 ^s .83	21 ^s .03
13	...	...	20 ^s .87	21 ^s .04
14	...	...	20 ^s .90	21 ^s .11
15	...	...	"	"
17	...	...	20 ^s .91	21 ^s .06
19	...	...	20 ^s .72	21 ^s .04
20	...	...	20 ^s .78	21 ^s .08
21	...	...	20 ^s .80	21 ^s .06
Moyennes	...	...	20 ^s .83	21 ^s .06
23	21 ^s .12	21 ^s .11	...	...
26	20 ^s .89	20 ^s .98	...	...
27	21 ^s .01	"	...	...
30	20 ^s .94	21 ^s .09	...	...
Nov. 9	21 ^s .00	21 ^s .14	...	...
13	20 ^s .84	20 ^s .98	...	...
Moyennes	20 ^s .97	21 ^s .06	...	...

L'écart entre les valeurs françaises et anglaises subsiste, mais les nombres moyens de B et D sont identiques, une fois l'influence du relai éliminée, tandis que la variation d'équation personnelle de L est de nouveau mise en évidence et fait croître à chaque période les nombres obtenus par H T et L.

Ce sont maintenant les valeurs françaises qui s'accordent entre elles, tandis que les nombres anglais sont discordants. Un tel résultat suffit pour enlever toute confiance aussi bien dans l'une que dans l'autre des valeurs de la longitude. Comme l'a fort bien dit l'astronome royal, il est nécessaire de recommencer la mesure et le plus tôt sera le mieux.

Il faudra, dans une nouvelle détermination, supprimer le relai distributeur, donner à chaque chronographe une pendule séparée, et surtout s'efforcer de s'affranchir des erreurs instrumentales (telles que : mouvements des objectifs, irrégularités des fioles des niveaux et flexions anormales), qui se sont manifestées, tant dans les instruments anglais que dans l'instrument français de Greenwich. Il y aurait, croyons-nous, une garantie sérieuse à échanger les instruments comme les observateurs. Il faut encore noter, comme l'a fait monsieur Turner dans l'*Observatory*, que les deux seules longitudes faites parallèlement et simultanément par deux couples d'observateurs différents ont donné toutes deux des résultats discordants. Peut-être la précision qu'on est habitué à attribuer aux longitudes télégraphiques n'est elle pas réelle. Les fermures des polygones jusqu'à présent publiées tendraient à le faire supposer.

*On the recent Determination of the Longitude Paris-Greenwich.
Reply to Colonel Bassot and Commandant Defforges. By
H. H. Turner, M.A., B.Sc.*

In my last paper (*Monthly Notices*, vol. li. No. 3, page 155) I pointed out that the independent determinations of the longitude Paris-Greenwich in 1888 by English and French observers did not agree, the original discordance amounting to nearly 0".4. I indicated my opinion that the determinations of level-error by the French observers at Greenwich with a striding level were, in some unexplained way, erroneous, and their determinations by reflexion observations from mercury were correct; on which supposition the value obtained for the longitude by the French observers would be in fair agreement with the English result. And I further appealed to the transit-circle observations at Greenwich to confirm this view.

I was aware that these opinions are not shared by the French observers, and should have added a statement of their views had I not hoped that they would make a direct communication on the subject, either in the publications of this Society or elsewhere. And I have now to thank them for the frankness with which they have stated their objections to my reasoning, and for

the honour they have done me in asking me to communicate the paper to the Society. The longitude Paris-Greenwich is one of such importance, and its determination was undertaken with such care and labour, that it seems to me imperative that any points at issue in connection with it should be clearly stated while yet they are fresh in the minds of those familiar with the work, in order that corresponding precautions may be taken in the future. And accordingly I do not hesitate to give in reply to the foregoing paper the reasons why I personally cannot accept the arguments therein contained.

These arguments are briefly as follow:—

(1) A comparison of personal equations shows that those of the French observers remained satisfactorily constant, while those of the English observers changed. This, as far as it goes, points to a greater accuracy in the French result.

(2) The daily results for time-determination at Greenwich, which I have compared with those obtained with the transit circle, differ so irregularly from the latter as to suggest a systematic error.

(3) Further, this systematic error may be localised in the relay interposed in the clock-circuit at Greenwich in order to convert make-signals into break-signals (to suit the French chronographs kindly lent for the occasion).

(4) Hence this comparison with the transit circle must be rejected, and we have thus no method of choosing between the two discordant results for longitude except that indicated in (1). The work must thus be done over again.

My reply to these paragraphs, in order, is as follows:—

(1) It is perfectly true that the personal equations of the English observers changed, as was very likely to happen in using small instruments for the first time, instead of the transit circle to which they were accustomed. But, owing to the special arrangements made for eliminating the effect of any such change, by a double interchange of observers, the effect on the longitude must be very small—far too small to explain any discordance such as is under consideration. Further, the accordance of the personal equations of the French observers with previous values proves nothing as to the accuracy of the longitude. The personal equations would have come out just as well if they had by some mistake observed a longitude any number of seconds, or minutes, or hours in error; that is to say, a systematic error in time-determination at one end of the line alters the longitude but not the personal equation.

Perhaps, however, the French observers may be referring more particularly to the suggestion in the last paragraph of my former paper—viz. that the error in level-determination to which I attribute the whole discordance was of such a nature as to affect the observations of B differently from those of D; and in this case they quite rightly point to the personal equations as evidence against this supposition. I should like to say one

or two words in examination of this point. In a letter to me dated 1891 February 5 Commt. Defforges says :—

“ Le Colonel Bassot a fait, le 18 Octobre, une série complète d’observations du nadir, qui donnent *pratiquement* le même résultat que les miennes. Voici d’ailleurs les nombres définitifs pour toute la longitude.

β niveau — β nadir.

	^s		^s
Sept. 30	+ 0°124	Oct. 6	+ 0°236
30	+ 0°121	18	+ 0°129†
Oct. 1	+ 0°199	18	+ 0°120†
2	+ 0°216	26	+ 0°269
3	+ 0°109	26	+ 0°248
5	+ 0°125	Nov. 13	+ 0°157
6	+ 0°183	13	+ 0°119

† “ Par Bassot : et sans aucune correction aux nombres observés comme vous l’aviez suggéré d’abord.”

“ Réflexion faite et ayant consulté plusieurs astronomes, nous avons cru devoir prendre, comme vous dites, la moyenne des valeurs données par l’inclinaison et le nadir. Mais vous voyez que la correction appliquée aux niveaux ne peut donc être la cause de la différence que vous signalez entre les longitudes déduites des observations au transit-circle et des observations de Paris.”

With respect to the note on October 18, I cannot find on referring to the original paper in which I drew up a list of these discordances any suggested alteration of the “nombres observés” on October 18, but there is a note that the results by striding level [β niveau] for that day were interpolated between those for October 17 and October 19, there being no direct observations on October 18. My data were obtained from the French computation books, the original note-books being in Paris; and no stellar observations having been made on October 18, the striding-level determinations were possibly omitted from these computations, but were recovered on referring to the original records. But I had clearly overlooked the fact that nadirs were taken by Colonel Bassot on this day, for I say in my former paper (p. 163): “It is to be remarked that the systematic correction to nadir observations was applied to the observations of B and D alike, although B did not make any nadir determinations. This may account for the discrepancy between their results.” I was thus wrong in saying that B did not make any nadir determinations, but I am not clear that his observations do not rather support my view than condemn it. The mean value of the discordance found by D is 0°176, and by B (on one day only) is 0°125. The difference is 0°051, and its effect on the longitude $0°051 \times \sec. 51^\circ = 0°08$, which reduces the discordance shown between B and D on p. 163 of my last paper from 0°20 to 0°12. And it is quite possible that the determination on October 18 would be more legitimately compared, not with D’s whole series,

but with the determinations on October 6 and 26, in which case the suggested difference between B and D would be much larger, and would amply explain the whole discordance.

But the simplest way of regarding the matter is to reconstruct Tableau I. of the preceding paper for the two cases :

1. When the observations of the French instrument at Greenwich are reduced with level-errors obtained with the striding level.

2. When they are reduced with level errors given by nadir observations, taking only those nights when nadir observations were made. For the middle series in this case, we have only the single night October 18 on which no stellar observations were made; but I have supposed this nadir determination to apply to October 17 and October 19, as the best we can do.

TABLEAU I.

<i>Striding Level.</i>					<i>Nadir.</i>			
	H T and L.	B and D.	H T and D.	B and L.	H T and L.	B and D.	H T and D.	B and L.
	s	s	s	s	s	s	s	s
Sept. 30	20·73	20·98	21·23	20·47	20·73	20·79	21·04	20·47
Oct. 1	20·77	21·02	21·25	20·56	20·77	20·71	20·94	20·56
3	20·98	21·12	21·32	20·72	20·98	20·95	21·15	20·72
5	20·97	21·04	21·27	20·73	20·97	20·84	21·07	20·73
Oct. 11	20·65	21·30	20·81	21·18	20·65	...	20·81	...
13	20·69	21·31	20·64	21·37	20·69	...	20·64	...
14	20·72	21·38	20·71	21·39	20·72	...	20·71	...
15	...	...	20·88	...	...	...	20·88	...
17	20·73	21·33	20·84	21·27	20·73	21·14	20·84	21·08
19	20·54	21·31	20·82	21·09	20·54	21·12	20·82	20·90
20	20·60	21·35	20·82	21·11	20·60	...	20·82	...
21	20·62	21·23	20·72	21·15	20·62	...	20·72	...
Oct. 23	21·30	21·10	21·45	20·96	21·30	...	...	20·96
26	21·07	20·97	21·20	20·72	21·07	20·56	20·79	20·72
27	21·19	...	...	...	21·19	...	...	...
30	21·12	21·08	21·36	20·89	21·12	...	...	20·89
Nov. 9	21·18	21·13	21·55	...	21·18	...	...	...
13	21·02	21·97	21·29	20·67	21·02	20·75	21·07	20·67
1st per.	20·86	21·04	21·27	20·62	20·86	20·83	21·05	20·62
2nd „	20·65	21·32	20·78	21·22	20·65	21·13	20·78	20·99
3rd „	21·15	21·05	21·37	20·81	21·15	20·66	20·93	20·81
1 and 2	20·76	21·18	21·02	20·92	20·76	20·98	20·92	20·81
2 and 3	20·90	21·19	21·08	21·02	20·90	20·90	20·86	20·90
Longde.	20·83	21·19	21·06	20·97	20·83	20·94	20·89	20·86

The results for longitude in the second case are thus moderately accordant, while those in the first are discordant. The accidental errors in the second case are of course larger, as the level-error depends on a single determination on each night instead of several.

The personal equations are as follow in the second case :—

HT-L _s	B-D _s	HT-D _s	B-L _s
-0'10	+0'15	-0'14	+0'19
-0'25	+0'24	-0'08	+0'09
-0'18	+0'19	-0'11	+0'14

in which there is nothing extravagantly discordant.

I do not insist that in view of the above systematic discrepancy between nadir and striding-level determinations one or other is clearly wrong; but it does seem to me that so large a systematic difference *suggests* some instrumental defect. The difference is larger than accidental differences should be. Did the observations stand alone, the only course would be to take the mean of the two series, as has been done in the French reductions. But the independent English operations seem to me to supply just the additional evidence required to enable us to choose between the two alternatives, instead of taking the mean.

(2) The differences of time-determination by the transit circle and by the longitude instruments are certainly somewhat irregular, but quite within the limits of what is possible, considering that different instruments and observers are compared. The parallelism of the four curves is not more pronounced than it should be, considering that one instrument (the transit circle) is compared with each of four others; and consequently its errors are common to the four curves. The obvious dip on October 26, for instance, is undoubtedly due to the transit-circle observations; and on careful scrutiny can be traced to a bad observation of *Polaris* by a young observer, from which the azimuth error on this day is deduced. It is to be remarked that the errors of the transit circle, though attributed to the observer A D, are really determined by many different observers, and reduced to A D by applying the assumed differences of personal equation.

(3) We now come to the real point at issue—the constancy of action of the relay. It is suggested that this time of action varied to the extent of some tenths of a second, according to the adjustment of the armature! If this were possible, it would make it almost impracticable to use relays in such work; but, fortunately, all the direct evidence available is against such an assumption.

In the first place, the indications of the sidereal standard through the relay on the longitude chronographs (E and F) were on many days directly compared with those without relay

on the Royal Observatory chronograph (R O) used in transit-circle observations. The following are the results :—

Date.	RO—E s	RO—F s
Sept. 29	+ 0·04	+ 0·05
Oct. 17	+ 0·08	+ 0·08
19	+ 0·06	+ 0·05
20	+ 0·02	...
21	+ 0·03	...
23*	— 0·02	...
27	+ 0·05	...
Nov. 13	+ 0·09	...

* On October 23 there is some evidence that one of the pens of chronograph E was loose.

These differences are not corrected for the “parallax of prickers” of R O. This quantity is about 0·04, but is neglected as constant in all transit-circle observations. Its sign is such as to diminish the above positive quantities, so that the time of action of the relay was always very small indeed. It is perhaps unfortunate that these comparisons were not made oftener, but it will be noticed that they were made in all three periods, and indicate no sensible difference in adjustment in the three periods, although the relay was adjusted by D or L in the first and third periods, and by B or H T in the middle period.

In the second place, if there were any fault in the relay, both French and English observations at Greenwich should be affected similarly. But nothing is clearer than that the fault, whatever it may be, affects the observations in opposite directions, or affects one and not the other. The difference between the time-determinations by B and H T, which was about — 0·28 at Paris during the first period, became — 0·43 at Greenwich during the second if the striding-level determination be accepted and returned to — 0·32 at Paris during the third. Similarly, the difference D — L was — 0·30 at Greenwich, became + 0·08 at Paris, and returned to — 0·12 at Greenwich.

If, then, we assume the Paris observations (against which nothing has been alleged by anybody) to be correct, the personal equations B—H T and D—L should be — 0·30 and + 0·08. But at Greenwich, if we accept the observations of striding level, these are found to be — 0·43 and — 0·21 respectively; and the action of the relay is here eliminated, for it was common to B and H T, and to D and L in the two cases. On the other hand, the nadir determinations, as far as they go, give — 0·35 and + 0·14.

For these two reasons, (a) that the time of action of the relay was, on all occasions on which it was tested, found to be

extremely small; (β) that its effect on time-determination was common to both observers at Greenwich, it does not seem to me that the discordance we are considering can be put down to the relay.

(4) The fourth paragraph in which I have summed up the paper of the French observers deals rather with matters of opinion. I have endeavoured in what precedes to keep mainly to facts. But, perhaps, I may say briefly that there does not seem to me to be any reason for rejecting the comparison with transit-circle observations, which was proposed by the Astronomer-Royal as an important item in the programme when the plan of the work was being discussed, and is a most valuable check on the results. Further, no clear reason has been given why the English result, $9^m 20^s.86$, should not be near the truth (for the admitted variation in personality cannot seriously affect the longitude), and there is a very simple supposition, as has been pointed out in the earlier part of this paper, which would bring the French result into accordance with this.

Observations of Phenomena of Jupiter's Satellites at Windsor, New South Wales, in the Year 1890. By John Tebbutt.

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Mr. Tebbutt, Observations of

LI. 6,

Day of Obs.	Satellite.	Phenomenon.	Phase.	Aperture of Telescope.	Mag. Power.	Greenwich Mean Time of Observation. h m s	Mean Time of Nautical Almanac. h m s
July 14	I.	Tr. Ingr.	Ext. contact	8-inch	230	0 17 25	0 19
14	I.	"	Bisection	"	"	0 19 5	
14	I.	"	Int. contact	"	"	0 21 5	
14	I.	Occ. R.	First seen	"	140	23 54 20	23 56
14	I.	"	Bisection	"	"	23 55 24	
14	I.	"	Last contact	"	"	23 56 59	
21	I.	Ecl. D.	Last seen	"	130	23 9 55	23 9 57
22	I.	Tr. Egr.	Int. contact	"	110	22 42 36	
22	I.	"	Bisection	"	"	22 45 46	
22	I.	"	Ext. contact	"	"	22 48 6	22 49
31	II.	Occ. D.	First contact	"	130	21 46 22	
31	II.	"	Bisection	"	"	21 49 26	
Aug. 13	IV.	Tr. Egr.	Bisection	"	110	20 17 52	20 26
13	IV.	"	Ext. contact	"	"	20 21 41	
13	I.	Occ. D.	First contact	"	170	22 54 50	
13	I.	"	Bisection	"	"	22 58 15	22 58
13	I.	"	Last seen	"	"	23 0 4	
14	I.	Tr. Ingr.	Ext. contact	"	130	20 5 52	
14	I.	"	Bisection	"	"	20 7 42	20 6
14	I.	"	Int. contact	"	"	20 10 37	

April 1891.

Phenomena of Jupiter's Satellites.

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Day of Obs.	Satellite.	Phenomenon.	Phase.	Aperture of Telescope.	Mag. Power.	Greenwich Mean Time of Observation. h m s	Mean Time of Nautical Almanac. h m s
Aug. 14	I.	Tr. Egr.	Int. contact	8-inch	130	22 22 29	22 26
14	I.	"	Bisection	"	"	22 24 34	
14	I.	"	Ext. contact	"	"	22 26 49	
15	I.	Ecl. R.	First seen	"	"	20 7 48	20 7 48
15	I.	"	Full brightness	"	"	20 10 22	
15	III.	"	First seen	"	"	20 46 47	20 47 41
16	II.	Tr. Ingr.	Ext. contact	"	"	21 15 9	
16	II.	"	Bisection	"	"	21 16 34	21 18
16	II.	"	Int. contact	"	"	21 18 44	
21	I.	"	Ext. contact	"	170	21 50 50	
21	I.	"	Bisection	"	"	21 52 54	21 51
21	I.	"	Int. contact	"	"	21 54 49	
21	IV.	Occ. D.	First contact	"	"	21 48 35	
21	IV.	"	Bisection	"	"	21 51 55	21 56
21	IV.	"	Last seen	"	"	21 55 24	
22	I.	Ecl. R.	First seen	"	"	22 2 50	
22	I.	"	Full brightness	"	"	22 5 1	22 2 51
23	III.	"	First seen	"	"	0 46 33	0 48 18
23	III.	"	Full brightness	"	"	0 52 53	

Day of Obs.	Satellite.	Phenomenon.	Phase.	Aperture of Telescope.	Mag. Power.	Greenwich Mean Time of Observation. h m s	Mean Time of Nautical Almanac. h m s
Aug. 25	II.	Ecl. R.	First seen	8-inch	170	21 50 53	21 51 28
25	II.	"	Full brightness	"	"	21 53 6	
Sept. 7	I.	"	First seen	"	"	20 22 6	
7	I.	"	Full brightness	"	"	20 24 22	
7	IV.	Ecl. D.	Last seen	"	"	21 24 42	
8	II.	Occ. D.	First contact	"	"	22 19 17	
8	II.	"	Bisection	"	"	22 21 22	
8	II.	"	Last seen	"	"	22 23 21	
14	I.	Ecl. R.	First seen	"	"	22 17 15	
14	I.	"	Full brightness	"	"	22 20 20	
15	IV.	Tr. Ingr.	Ext. contact	"	"	21 25 26	
15	IV.	"	Bisection	"	"	21 30 25	
15	IV.	"	Int. contact	"	"	21 35 14	
16	III.	Tr. Egr.	Int. contact	"	"	22 27 42	
16	III.	"	Bisection	"	"	22 30 22	
16	III.	"	Ext. contact	"	"	22 35 11	
22	I.	"	Int. contact	"	"	20 9 35	
22	I.	"	Bisection	"	"	20 12 54	
22	I.	"	Ext. contact	"	"	20 15 14	

April 1891.

Phenomena of Jupiter's Satellites.

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Day of Obs.	Satellite.	Phenomenon.	Phase.	Aperture of Telescope.	Mag. Power.	Greenwich Mean Time of Observation. h m s	Mean Time of Nautical Almanac. h m s
Sept. 26	II.	Ecl. R.	First seen	8-inch	170	21 36 35	21 37
26	II.	"	Full brightness	"	"	21 40 15	
27	III.	"	First seen	"	"	20 51 28	
27	III.	"	Full brightness	"	"	20 55 45	
28	I.	Occ. D.	First contact	"	"	22 36 32	20 53 56
28	I.	"	Bisection	"	"	22 38 41	22 38
28	I.	"	Last seen	"	"	22 40 6	
29	I.	Tr. Egr.	Bisection	"	"	22 3 34	
29	I.	"	Ext. contact	"	"	22 7 18	22 6
Oct. 4	II.	Ecl. R.	First seen	"	"	0 14 3	0 14 23
4	II.	"	Full brightness	"	"	0 17 11	
4	III.	Ecl. D.	Began to fade	"	"	21 15 44	
4	III.	"	Last seen	"	"	21 22 25	21 22 48
5	III.	Ecl. R.	First seen	"	"	0 53 13	0 55 30
5	III.	"	Full brightness	"	"	0 59 2	
7	I.	"	First seen	"	"	22 32 11	22 32 6
7	I.	"	Full brightness	"	"	22 34 50	
21	I.	Occ. D.	First contact	"	"	22 44 18	
21	I.	"	Bisection	"	"	22 46 48	22 45
21	I.	"	Last seen	"	"	22 48 7	

Day of Obs.	Satellite.	Phenomenon.	Phase.	Aperture of Telescope.	Mag. Power.	Greenwich Mean Time of Observation. h m s	Mean Time of Nautical Almanac. h m s
Oct. 22	I.	Tr. Egr.	Int. contact	8-inch	170	22 9 5	22 14
22	I.	"	Bisection	"	"	22 11 59	
22	I.	"	Ext. contact	"	"	22 14 4	
23	I.	Ecl. R.	First seen	"	130	20 51 45	
23	I.	"	Full brightness	"	"	20 55 13	20 51 43
26	II.	Tr. Ingr.	Ext. contact	"	170	21 40 9	
26	II.	"	Bisection	"	"	21 41 54	21 37
26	II.	"	Int. contact	"	"	21 44 23	
28	II.	Ecl. R.	First seen	"	140	21 26 19	
28	II.	"	Full brightness	"	"	21 29 49	21 26 14
30	I.	"	First seen	"	"	22 47 16	
30	I.	"	Full brightness	"	"	22 49 29	22 47 4
Nov. 5	III.	Tr. Ingr.	Ext. contact	4½-inch	180	22 2 45	
5	III.	"	Bisection	"	"	22 4 59	22 2
5	III.	"	Int. contact	"	"	22 8 29	
6	I.	Occ. D.	First contact	8-inch	170	21 5 58	
6	I.	"	Bisection	"	"	21 8 12	21 6
6	I.	"	Last seen	"	"	21 8 37	
13	IV.	Ecl. D.	Last seen	"	140	22 4 20	22 9 49

Remarks.

July 14.—Definition pretty good.

July 21.—Definition pretty good, but images rather tremulous. Satellite disappeared close to the planet's limb, and was very faint for a long time before final extinction. Observation not good.

July 22.—Definition fair and images pretty steady.

July 31.—Definition pretty good, but final disappearance not seen, in consequence of cloud.

August 13.—Definition not good at bisection of IV., but better at external contact; bisection observation certainly late. Bad definition in case of I.; the satellite distinguishable through the planet's edge till almost the time of final disappearance.

August 14.—Definition bad at ingress, and the observation of external contact too late; definition pretty good at egress.

August 15.—Images tremulous at reappearance of I., but more steady and the definition pretty good at that of III. Sky clear. Full brightness of I. unsatisfactory. III. came out nearly in contact with I., and its reappearance could not therefore be accurately observed.

August 16.—Images steady and fair definition.

August 21.—Definition excellent, and observations unusually good.

August 22.—Sky beautifully clear. Images steady, and definition pretty good. Moon present, and observation of first phase rather late.

August 23.—Sky still clear, and definition excellent. Satellite conspicuous at recorded time, and the observation therefore too late.

August 25.—Sky beautifully clear and definition fair, but Moon not far from planet.

September 7.—Sky beautifully clear, but images tremulous and badly defined. IV. disappeared and reappeared several times before final extinction at the recorded time.

September 8.—Good definition. Satellite visible through limb till near final disappearance.

September 14.—Sky clear. Images pretty steady, and definition fair. Satellite suspected 4.5 seconds earlier and distinct at recorded time.

September 15.—Definition fair at first two phases, but much disturbed at the last. Satellite remarkably faint before ingress, and 29 minutes after internal contact it was plainly seen as an elongated dark spot.

September 16.—Images steady and definition excellent.

September 22.—Sky clear and definition good.

September 26.—Sky clear near planet, and definition pretty good.

September 27.—Sky beautifully clear; images steady and definition pretty good. Observation of first phase probably a second late. Full Moon had risen.

September 28.—Definition pretty good, but final disappearance difficult to observe.

September 29.—Bad definition.

October 4.—Sky clear throughout, but images tremulous and badly defined for II., and definition pretty good for III.

October 5.—Sky still clear, but definition not quite so good. Satellite suspected 17.5 seconds earlier.

October 7.—Sky clear and definition pretty good.

October 21.—Bad definition.

October 22.—Bad definition.

October 23.—Sky beautifully clear, and definition pretty good. Satellite suspected three seconds earlier.

October 26.—Fair definition.

October 28.—Sky clear, and images well defined. Satellite suspected 1.5 second earlier.

October 30.—Sky clear, and images steady and well defined.

November 5, 6.—Images steady and well defined, but very difficult to observe the last phase of I.

November 13.—Sky very clear, but definition very bad. Satellite had already faded considerably at 22^h 0^m 55^s. Satellite glimpsed at intervals till the recorded time, and was not afterwards seen.

NOTE.—An occulting bar was not employed in the eclipse observations. The times given in the first and seventh columns are the Windsor mean times of observation, diminished by 10^h 3^m 20^s·5, and entered to the nearest second.

Windsor, N. S. Wales:

1891 February 11.

Ephemeris for Physical Observations of the Moon. By A. Marth.
1891 May 11 to August 31.

Greenwich Noon.	Selenographical Colong. Lat. of the Sun.		Geocentric Libration. Sel. Long. Lat. of the Earth.		Combined Amount.	Direction.
1891.						
May 11	305°34	—0°43	+5°65	—2°97	6°37	242°2
12	317°57	0°41	5°30	4°26	6°79	231°1
13	329°80	0°38	4°64	5°31	7°04	221°0
14	342°02	0°36	3°73	6°09	7°13	211°3
15	354°24	0°33	°63	6°59	7°09	201°6
16	6°45	0°30	1°41	6°80	6°94	191°7
17	18°66	—0°28	+0°15	—6°72	6°72	181°3
18	30°87	0°25	—1°08	6°34	6°44	170°3
19	43°07	0°22	2°23	5°69	6°11	158°6
20	55°26	0°19	3°24	4°78	5°77	145°9
21	67°45	0°16	4°06	3°63	5°44	131°8
22	79°64	0°13	4°66	2°28	5°19	116°1
23	91°82	0°10	5°02	—0°79	5°08	98°9
24	104°00	—0°07	—5°12	+0°77	5°18	81°4
25	116°19	0°04	4°97	2°32	5°48	64°9
26	128°38	—0°01	4°57	3°77	5°92	50°4
27	140°58	+0°02	3°96	5°01	6°40	38°2
28	152°78	0°05	3°15	5°98	6°76	27°7
29	164°98	0°08	2°20	6°60	6°95	18°4
30	177°29	0°11	1°15	6°81	6°90	9°5
31	189°41	+0°14	—0°04	+6°62	6°62	0°4
June 1	201°64	0°16	+1°06	6°01	6°11	350°0
2	213°88	+0°19	+2°12	+5°04	5°47	337°2
June 10	311°86	+0°37	+3°63	—5°86	6°89	211°7
11	324°10	0°40	2°72	6°45	6°99	202°7
12	336°33	0°42	1°63	6°75	6°95	193°5

April 1891.

Observations of the Moon.

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Greenwich Noon.	Selenographical		Geocentric Libration.		Combined Amount.	Direc- tion.
	Colong. of the Sun.	Lat.	Sel. Long. of the Earth.	Lat.		
1891.						
June 13	348°56	+0°45	+0°43	-6°76	6°77	183°6
14	0°79	+0°47	-0°83	-6°47	6°52	172°7
15	13°01	0°50	2°08	5°90	6°25	160°7
16	25°23	0°53	3°23	5°07	6°01	147°6
17	37°43	0°56	4°22	4°01	5°81	133°6
18	49°63	0°58	4°97	2°73	5°67	118°8
19	61°83	0°61	5°44	-1°29	5°59	103°4
20	74°02	0°64	5°57	+0°25	5°58	87°4
21	86°22	+0°67	-5°36	+1°82	5°66	71°2
22	98°41	0°70	4°81	3°32	5°84	55°3
23	110°60	0°73	3°96	4°65	6°10	40°3
24	122°79	0°75	2°86	5°71	6°39	26°5
25	134°99	0°78	1°62	6°42	6°62	14°1
26	147°19	0°80	-0°31	6°73	6°74	2°6
27	159°40	0°82	+0°95	6°62	6°68	351°8
28	171°62	+0°84	+2°11	+6°09	6°44	341°0
29	183°84	0°86	3°10	5°19	6°05	329°3
30	196°07	0°88	3°89	4°00	5°58	315°9
July 1	208°31	0°90	4°47	2°59	5°17	300°1
2	220°55	+0°92	+4°84	+1°05	4°96	282°3
July 10	318°55	+1°07	+0°78	-6°67	6°71	186°7
11	330°79	1°09	-0°45	6°45	6°47	176°0
12	343°02	+1°11	-1°74	-5°96	6°21	163°7
13	355°25	1°13	3°02	5°21	6°02	149°9
14	7°47	1°15	4°22	4°23	5°97	135°2
15	19°69	1°17	5°24	3°04	6°06	120°2
16	31°90	1°19	6°01	1°69	6°24	105°7
17	44°11	1°21	6°44	-0°21	6°45	91°9
18	56°31	1°23	6°48	+1°31	6°61	78°5
19	68°50	+1°24	-6°08	+2°81	6°70	65°1
20	80°69	1°26	5°25	4°19	6°71	51°3
21	92°88	1°28	4°02	5°34	6°67	36°9
22	105°07	1°30	2°48	6°16	6°63	21°9
23	117°26	1°31	-0°78	6°57	6°62	6°7
24	129°45	1°33	+0°94	6°55	6°61	351°9
25	141°65	1°34	2°53	6°09	6°59	337°5
26	153°86	+1°35	+3°88	+5°24	6°52	323°6
27	166°07	1°36	4°93	4°08	6°39	309°7

Greenwich Noon.	Selenographical		Geocentric Libration.		Combined Amount.	Direc- tion.
	Colong. of the Sun.	Lat. of the Sun.	Sel. Long. of the Earth.	Lat. of the Earth.		
1891. July 28	178°29	+ 1°37	5°64	+ 2°71	6°25	295°7
29	190°51	1°38	6°03	+ 1°21	6°15	281°3
30	202°74	1°39	6°13	- 0°33	6°14	266°9
31	214°97	1°40	5°98	1°82	6°25	253°1
Aug. 1	227°21	+ 1°41	+ 5°64	- 3°18	6°48	240°5
Aug. 9	325°17	+ 1°47	- 2°34	- 5°23	5°73	155°9
10	337°40	1°48	3°66	4°30	5°65	139°6
11	349°62	1°49	4°90	3°17	5°84	123°0
12	1°84	1°50	5°98	1°89	6°27	107°6
13	14°06	1°51	6°81	- 0°49	6°82	94°1
14	26°26	1°51	7°29	+ 0°97	7°36	82°4
15	38°46	1°52	7°36	2°43	7°75	71°7
16	50°66	+ 1°53	- 6°93	+ 3°80	7°90	61°2
17	62°85	1°53	6°00	4°99	7°80	50°1
18	75°03	1°54	4°59	5°90	7°47	37°8
19	87°21	1°54	2°80	6°43	7°01	23°5
20	99°39	1°54	- 0°78	6°52	6°56	6°8
21	111°57	1°54	+ 1°28	6°15	6°28	348°3
22	123°75	1°54	3°20	5°36	6°24	329°2
23	135°94	+ 1°54	+ 4°82	+ 4°22	6°40	311°2
24	148°13	1°54	6°05	2°83	6°68	295°1
25	160°33	1°54	6°85	+ 1°31	6°98	280°8
26	172°54	1°53	7°24	- 0°25	7°24	268°0
27	184°75	1°53	7°25	1°75	7°46	256°4
28	196°97	1°53	6°95	3°12	7°62	245°7
29	209°19	1°52	6°40	4°31	7°71	235°9
30	221°42	1°52	5°65	5°27	7°72	226°9
31	233°65	+ 1°51	+ 4°75	- 5°97	7°62	218°3